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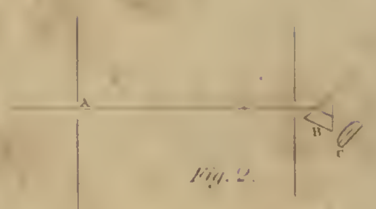

Fig. 2.

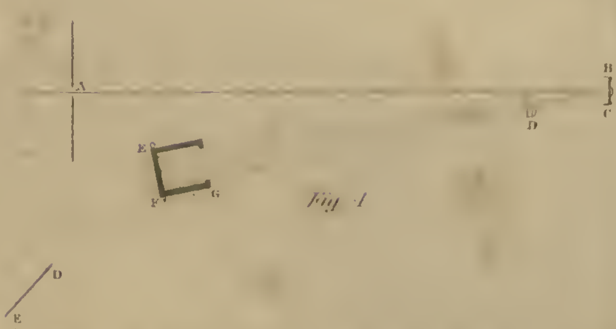

Fig. 4.

Fig. 1.

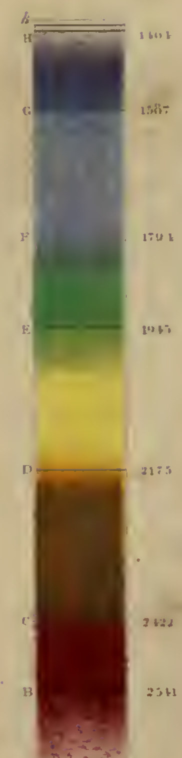

Fig. 3.

A TREATISE

ON

THE FORCES WHICH PRODUCE

THE

ORGANIZATION OF PLANTS.

WITH AN APPENDIX,

CONTAINING SEVERAL MEMOIRS ON CAPILLARY ATTRACTION, ELECTRICITY, AND THE CHEMICAL ACTION OF LIGHT.

BY

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PREFACE.

In the autumn of 1833, during a period of convalescence from a severe attack of the fever of the Southern States, my thoughts were first turned to the connexion of Chemistry with Vegetable Physiology.

At that time, as the works on Chemistry show, comparatively but little advance had been made in this interesting department of science. In the most popular treatises, Organic Chemistry consisted chiefly of a descriptive history of the acids and bases of the vegetable and animal kingdoms.

It is impossible for any one to witness in those warm climates the rapidity with which all the operations of vegetable life are carried on, without having his thoughts directed to the obvious connexion which exists between these phenomena and external agents. Even in the opinion of the uneducated planter, the rapidity of vegetable growth is connected with the warmth of the season, and the occurrence of favourable rains, and the brilliancy of the sun.

A seed which has been buried in the ground a few days, makes its appearance above the surface. It soon puts forth its leaves, which turn green in the light, and becomes an active laboratory of all kinds of chemical and mechanical processes. Starch, gum, sugar, and a variety of other substances are formed from inorganic matter, water is drawn up in large quantities from the ground, and evaporated from the leaves.

All these phenomena unquestionably depend on the common laws of physics. Our interest in them is greatly increased by the close resemblance of many of them to things taking place in the case of animal systems. The productions of vegetable life are, many of them, also apparently the productions of animal life. The physical processes which appear in one of the great classes of living beings, appear in the other too. Inquiries into the nature of the vital processes of plants end in resolving problems connected with the physiology of animals.

In every plant there are two prominent operations carried forward: the production of organic matter, and its distribution through the various parts of the vegetable system. It is to the consideration of these that the following pages are chiefly devoted. As to the various transformations which take place in the interior of these organisms, and the chemical principles involved in those changes, anything which I might offer would be insignificant, compared with the splendid results which have been obtained by the German, French, and English chemists during the period which has elapsed since these researches were first undertaken.

Any one who peruses the works on Botany and Vegetable Physiology must be struck with the inexact views which are entertained on the mode of action of the solar rays in producing the green colour of plants, and effecting the decomposition of carbonic acid gas. Yet this is unquestionably the most remarkable result in physiological and physical science. It is the transmutation of inorganic into organized matter. With a view of giving clearness and precision to those vague notions, I have devoted much of the following work to this point, showing how, as plants grow in water or in air, they are furnished, upon physical principles, with nutrient material; how, upon this, the sun-

beam acts, and how, of the rays of which that beam is composed, the yellow ray of light is the operative principle. The mode by which the resulting decomposition is effected leads to an investigation of the absorption of light.

As will be seen from the Appendix, in these investigations I have not restricted myself exclusively to the subjects before me, but have, in most instances, followed them out into their physical details. The chapters of the Appendix have all been published in various American and foreign works, each chapter being a memoir, more or less complete, on the topic on which it treats. In that Appendix, therefore, there will be found a large amount of experimental matter, of which an immediate use is not made. But in philosophy no new facts are superfluous. At first sight, there might seem to be no connexion between the fixed lines of the solar spectrum and the production of chlorophyl, yet, as will be hereafter understood, very great use may be made of the former in investigations on the nature of the latter.

The remark which has just been made respecting the vague ideas which are entertained in relation to the mode of action of light, might also have been made in relation to capillary attraction. This force, with exosmose and endosmose, are very favourite resources of the botanist in his difficulties. In the chapters in which these topics are considered, I have endeavoured to show the true relation which exists between endosmosis and capillary attraction, and how the latter itself springs from electric excitement. I have shown how, in strict conformity with this principle, the flow of the sap and the circulation of the blood may be explained; the systemic circulation, for example, arising as a necessary consequence of the deoxydation of arterial blood.

The theory of these circulatory movements taking place in organized structures I have not heretofore published, though they have been taught in my lectures in this University. It will be seen that the peculiarity is in tracing the action to chemical changes; that the systemic circulation arises, as has just been said, from the deoxydation of arterial blood; the pulmonary circulation from the oxydation of venous blood; and the flow of sap in flowering plants from the carbonization of water by the sunlight in their leaves.

It might be supposed that Chapter XVIII., Ap., is intended as a reply to the criticisms on some parts of Chapter XIII., which have been published by M. Edmond Becquerel, in the *Annales de Chimie*. When that chapter was written, I had not, however, had the opportunity of seeing his valuable memoir. The experiments there given were not made with the iodide, but the bromide of silver, a circumstance which relieves me from much of the weight of the criticisms of that chemist. It is not only from my own experiments, but also from those of Sir J. Herschel, that I am led to differ from M. Becquerel, in regarding the iodide and bromide of silver as exhibiting very different changes in the spectrum. M. Becquerel's idea that there is but little difference between them, does not seem to be consonant to experiment.

Of the Appendix, several chapters have already been translated into different European languages, and a variety of criticisms upon them have been published. It is this circumstance which has caused me to hasten the printing of the whole of these papers in this collected form now offered to the public. Whatever care be taken in the preparation of such translations, it will necessarily happen, that on many points mistakes will be made as to the meaning and views of an author. This has happened in the present instance, both in Germany and France: for example, in Chapter XV., which has been republished in Paris in the *Annales de Chimie*, errors of the kind occur. Those who are familiar with these matters will find, when they look over these papers in the form

now given, that views have been thus accidentally imputed to the author which he does not entertain.

It did not enter into my plan to reprint any of the valuable memoirs which have been published by other chemists on these topics, more especially on those relating to the chemical agencies of light. I may, however, direct the attention of the readers to a description by Sir J. Herschel, of the spectrum alluded to in the Appendix (645). It is inserted in the Philosophical Magazine for February, 1843. In this paper will be found what probably will prove to be the true theory of the action of light on Daguerreotype plates, and the reader of this volume will be the more interested in it, inasmuch as it contains criticisms on several of the leading doctrines here set forth.

The different chapters of the Appendix are, for the most part, reprinted from sources mentioned under the title of each. In a few instances slight changes have been made in them; changes which have been rendered necessary by the mode in which they were originally published. Thus some of these papers were printed in various American, and others in European journals, and it sometimes happens that experiments are in consequence described twice over. This repetition I have avoided by removing the paragraphs involved. In this manner I have entirely omitted to republish in this work a paper on the constitution of the atmosphere, given in the Philosophical Magazine for October, 1838, because nearly all its experimental matter had been previously published in the American Journal of Medical Sciences, and is given in Chapter VI.

The time has now arrived when both Vegetable and Animal Physiology are to have their foundations laid on Chemistry and Natural Philosophy, the only basis which can elevate them from their present deplorable position to that of true sciences. It is with this impression that the explanations which I have given in this book of the mode by which light acts in determining organization, and of the mechanical causes by which such organized matter is transmitted from point to point of living systems (for these are the leading facts which this work is designed to illustrate), are offered to the attention of chemical philosophers.

John William Draper.

University of New-York, October 1, 1844.

N.B.—The references to the Appendix are made by the number of the paragraph, thus (Ap., 100, 101). When the place referred to is in the body of the work, the letters Ap. are omitted, thus (100, 101).

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INTRODUCTION.

GENERAL REMARKS ON THE INFLUENCE OF PHYSICAL AGENTS ON ORGANIZATION AND LIFE.

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1. THE rapid progress of organic chemistry has recently drawn the attention of scientific men to many remarkable relations which exist between animated beings and the inorganic world. For a long period, physiological doctrines, the spirit of which, for many ages, has undergone but little change, offered an insurmountable barrier to the application of methods of physical research to problems connected with the phenomena of life. On these, in our times, a successful inroad has been made, chiefly through the aid of improved methods of chemical analysis. In a philosophical point of view, it was the office of the seventeenth century to unfold the doctrine of universal gravitation, to assign proper causes for the motions of the celestial bodies, and to develop the great doctrines of astronomy. It was the office of the eighteenth to lay the foundations of physics and chemistry, or of that group of sciences which embraces the relations and reactions of atoms. It is the office of the nineteenth to discover the laws which obtain in the complicated structure of animated beings, those laws which give rise to the mysterious phenomena which we call life.

2. This book, in which will be found some facts which it has happened to its author to discover, is offered as an humble contribution among those more brilliant gifts with which Germany, and France, and England have lately enriched vegetable and animal physiology. It treats of a subject which forms the connecting link between pure chemistry and those higher sciences. The great idea which it is designed to illustrate is

that which connects the production and phenomena of organized beings with the imponderable principles.

3. In this work the existence of the Vital Force of physiologists—as a homogeneous and separate force—is uniformly denied. The progress of science shows plainly that living structures, far from being the product of one such homogeneous power, are rather the resultants of the action of a multitude of natural forces. Gravity, cohesion, elasticity, the agency of the imponderables, and all other powers which operate both on masses and atoms, are called into action, and hence it is that the very evolution of a living form depends on the condition that all these various agents conspire. There is no mystery in animated beings which time will not at last reveal. It is astonishing that, in our days, the ancient system, which excludes all connexion with natural philosophy and chemistry, and depends on the fictitious aid of a visionary force, should continue to exist: a system which, at the outset, ought to have been broken down by the most common considerations, such as those connected with the mechanical principles involved in the bony skeleton, the optical principles in the construction of the eye, or the hydraulic action of the valves of the heart.

4. In their origin, all those important ideas which now constitute modern science have been obscurely and imperfectly set forth. It is not given to the human mind, when it emerges from the darkness of ignorance, any more than to the human eye, when it emerges from physical darkness into the sunshine, to see all objects which are before it in their proper aspect and position. A period of time must elapse, during which we become accustomed to the light. Future discovery, in its progress, may show that, of the facts brought forward in this volume, many are misplaced, and many misapplied; these are incidents to which all philosophical works are liable. But if it should happen that anything contained herein shall aid in fastening the attention of men of science on the idea which it is designed to impart, the author will have received his reward, and the labours of ten years will not have been entirely thrown away.

5. Organized beings and organized bodies spring forth in those positions only to which the rays of the sun have access. They are, therefore, limited to the atmosphere, the sea, and the surface of the earth. Periodical vicissitudes, which are observed both in vegetables and in animals, serve to show that this is not a mere fortuitous coincidence, but rather an intimate connexion between the phenomena of life and the presence of the imponderables. When the sun is set, the leaves of plants no longer decompose the carbonic acid of the air, but a pause takes place in the activity of their functions, and they sink into a passive condition. The gaseous bodies brought from the ground by the action of the spongioles percolate through the delicate tissues of the leaf, and escape away into the atmosphere. At night, also, in many flowers, the petals fold themselves together, and, for a time, all active processes cease. It is, therefore, through an instinctive impulse, that comes over them during this period, that all animals, except such as take their prey by night, seek places of rest. Darkness, and silence, and repose are all connected together.

6. It is one of the greatest discoveries of the present age, that the races of animals which have inhabited our globe were of successive creation; that they constitute a

series, the extreme terms of which bear no resemblance to each other; that, commencing with those of the earliest date, we are able to trace a constant progress both in intellectual and structural development. In all, there are found evidences of the operation of the same formative power, acting by and resorting to the aid of the same physical principles. The trilobite, of the primary fossiliferous rocks, and man, of the most recent, have, at the first glance, little in common, but a more attentive observation soon shows that the idea of construction in each is so allied, that they have both undoubtedly sprung from the operations of the same Intelligent Mind.

7. Do, then, these successive races of sentient beings form altogether a strictly continuous series? As in the series of mathematicians, where each term bears a definite relation to those which precede it, and contains within itself the elemental law of those which are to come after it, do each one of these organized beings observe a position of relationship with those that are of earlier, and those also which are of later date? Do the animals and the plants of the Carboniferous Period connect those of the Silurian with those of the Newer Pleiocene? Were the organized mechanisms of the Old Red Sandstone essential to the appearance and existence of those which live with us? Are we, in short, to regard the Author of these wonderful forms as operating in each one of these instances by the same law, and, from small beginnings, evolving and transforming the most elaborate by a successive passage through those which are inferior? or are we to understand that, at particular and unconnected epochs, the broad hand of an overruling Providence is to be discovered, fashioning and framing each class of created forms, irrespective of external physical forces or agents, and giving birth spontaneously to unconnected tribes of animals and plants, which bear no sort of relationship to one another, and are not parts of one common plan in which there is a unity of design?

8. The interior movements of the solar system, and the collapsing of nebular masses, are committed to secondary agents or to immutable laws; and these are events which, for their completion, often require great periods of time. No invisible or extraneous agency ever intervenes. In her predestined course, the moon revolves and exhibits her phases, and, like the beating pendulum, though ten thousand years may have elapsed since its last beat, the comet—the pendulum of the universe—swings punctually past the sun. That universe is not an unchangeable mass, but is made up of moving and revolving orbs, which are all obedient to one common law. Do not, therefore, such things point out, that, in the midst of all these transitory affairs, immutable principles are involved, and a common law is incessantly in operation?

9. In the history of the human race, it may be observed that epochs have occurred, which, following each other with a kind of periodicity, have stood in relationship with, or even brought about, the conditions of modern civilization. As in the course of the life of an individual there is no incident which is not in connexion with circumstances which have preceded it, and none that does not give a bent to those that follow, we naturally view the vicissitudes of our existence as bearing the relation of cause and effect. We trace the circumstances of to-day from the circumstances of yesterday. In the movements of the celestial bodies we continually see fixed events resulting from the operation of apparently variable causes; the waxing and waning of the moon, the

eclipses of the sun, the transitory visits of comets. On our own earth we also witness oceanic tides which ebb and flow, and spring and summer, and autumn and winter, following each other unceasingly. From year to year we witness the alternate increase and shortening of the day, the tempests of the equinoxes, and the sultry weather of midsummer. In the world of organization which is around us, we observe similar mutations: there are plants which come up in spring, and die away in autumn; and even those hardier races which witness the changing of empires, give tokens that they are included within this law of variation. The oak unfolds its buds into leaves, which periodically fall to the ground. Among sensitive beings, from time to time, different races of animals have held dominion of the earth: at one period it has been almost the exclusive abode of reptiles, at another of four-footed mammalians, and at last is under the control of two-handed man.

10. In whatever direction, therefore, we look, we perceive the transitory nature of all things: even with those which, from their magnitude, their remoteness, or their importance, might with apparent propriety be regarded as not participating in these unceasing changes, the law holds good. Throughout the universe there is no monument that retains its primordial condition.

11. Each one of these various changes, no matter whether it concerns organic or inorganic nature, has been the result of the action of some determining cause. The countless systems of phenomena which have arisen are all connected together as systems of effects. In a web, as it passes from the loom, the different threads interlace with one another, and though we soon cease to identify each as it pursues its sinuous way, we know that the last is connected with the first; and in the web of nature each event has been brought into relation with others that have gone before it, and others that have succeeded it, and all are intertwined together as a series of causes and effects.

12. Understanding thus that no effect takes place except by the operation of some prior cause, we occupy ourselves in discovering its dependances on things that have preceded it. It is this which engages the contemplation of the greatest philosophers in their speculations on natural phenomena, and ordinary men in the daily affairs of life. The disasters of to-day we attribute to the errors of yesterday, and the possession of glory, and wealth, and position in society, to plans that have been conducted well. No man is in heart a fatalist, for each one clearly perceives that his destinies are in his own charge; and no small portion of human happiness or misery springs from a knowledge of these things. In other tribes of life, where intellectual processes are replaced by processes of instinct, it is very different: the wild animal which lives in the prairies meets seasons of famine and distress without a moan. Careless for to-morrow, he bears up with his present lot, and is utterly unconscious that he may have been the author of his own wo. There is with him no recording memory of whence he came, no distracting care of whither he may go. He acts, in respect of the passing time, in the same way that men act in respect of their whole life; they are ignorant of what happened in their early days of infancy, and never trouble themselves about what may come in old age.

13. The vegetable world, from possessing no nervous system, is inherently incapable

of appreciating its own existence, and much more, therefore, of feeling emotions of pleasure and pain. To animals nature has given a present contentment, which is purchased by ignorance; but to men, who are endowed with reasoning powers, whose nervous system has been so formed as to enable their mental operations, by processes of memory, to reach over long periods of time, to combine together events which are afar off, to decompose into their constituent parts phenomena that are complex, and to trace each one of those parts up to its proper cause, knowledge has been given, and the price of that knowledge is pain.

14. So far as their intellectual powers are concerned, the life of animals, even of those of the highest orders, is analogous to the dreamy sleep of man. Overcome by the toils of the day, the weary labourer sinks into repose, and there come before him pageants and scenery connected, to a certain extent, with the external world; but of that world he is wholly unconscious. Instead of the accustomed forms that he meets in his daily affairs, there spring up light aerial shapes and phantasms. From recesses in the brain, where they have been long stored, and perhaps forgotten, the recollection of landscapes that he has seen of old come forth; he views the well-known forms unfold themselves before him: there stands the aged oak, at the foot of which he has so often watched the setting of the sun, and there is the pale-blue sky with its gilded clouds, and in the distance the almost invisible mountains. That fairy panorama has its shadowy tenants, which live, and move, and breathe: the dead are also there. From those silent sepulchres which are within the brain, they rise again as living things, and people the scenes they once loved. They converse with and counsel the dreamer.

15. As thus, during the night, these phantoms come up spontaneously before us, and spontaneously disappear, and time, of which men gain a knowledge only by comparing events, passes unnoticed away, we see to what a small extent the will controls these phenomena. The spectres come unbidden, and they as suddenly depart, and very often, during one slumber, they change and rechange again, and memory, the arch-conjuror, evokes scene after scene.

16. With the brute creation the same thing holds. In their daily relations with external nature, almost all their functions are carried on by the promptings of instinct, or in a mechanical way. So far as we can see, the current of thought seems to be little under their control, and they have no power of effecting the collocation of ideas. They cannot tell the passing of time, which flows away in its silent lapse, and leaves them, as it found them, contented. Unaided by the instincts which are implanted in them, they show but little power of reasoning from causes to effects, or from effects to causes. Even in the most obvious cases, where actions are performed before them which, if performed by themselves, might tend greatly to their enjoyment, they exhibit but a low imitative power. Though he has often seen it done by man, the monkey has never yet learned to make a fire.

17. All objects which surround us, whether animate or inanimate, are marked by a transitory nature. They come into existence, for a while they continue, and then they pass away. It is thus that, in the course of ages, the configuration of continents and

seas undergoes change—changes, however, which require long periods of time, and which, in the course of human existence, are, for the most part, imperceptible. Commencing with the inorganic world, which we see is included in this law of unceasing variation, we discover that each of its component structures passes through its transmutations more rapidly according as its constitution is more complicated. For the same reason it is that bodies which are organized—and organization in itself implies complicated structure—are of all forms most liable to these mutations. The dead carcass of an animal speedily disappears under the forms of water, ammonia, and carbonic acid, its elemental atoms breaking up into simpler and more enduring groups. It is a vulgar error that a living being possesses a principle of resistance to external agents, while a dead one submits itself to them; both equally change, or, of the two, the living one putrefies and changes the more rapidly; but, then, for each of the several systems of dead and dissevered atoms, appointed routes of passage are prepared. The carbonic acid escapes by the lungs, the nitrogenized compounds through the kidneys, and water through both those organs and the skin. The putrefaction of an organized being is a constant event; it commences before birth, and continues after death; but, though constant, it is regulated in life, and after death the avenues of discharge are all closed, and the mechanism appointed for removing the decaying atoms broken down.

18. Time, therefore, enters as an element in animal life. Individuals, after the progress of a few years, pass away, and, during each moment of their existence, their various parts are undergoing incessant change. There is a constant removal of all the carbon compounds from every part of the system; a removal which necessarily arises in conducting locomotion, and various other functions. If an electric current is to be passed along the wire of a voltaic battery, and is required to evolve a certain amount of light or heat, or to produce a certain amount of electro-magnetic motion, a fixed amount of zinc must be consumed. If a steam engine has a given quantity of work to perform, a given quantity of coal must be burned. So also in animal systems, the production of motion can only be effected by the consumption of the parts of the animal machine. In the higher races, in which an elaborate development of these functions has been accomplished, the processes of transmutation go on with the greatest rapidity. Among insects, which are constantly upon the wing, the combustion of the organic atoms is at a maximum, as is also, consequently, the production of heat; but at night, or when they rest, the rate of respiration diminishes, the heat declines, and the transmutation is checked. In the existence of an animal, as also in the existence of its constituent atoms, time, therefore, enters as an element.

19. There is a constant washing away of mountains into the sea, and rivers continually tend to fill up their beds. In one region the detritus of the land, brought down by streams, encroaches on the ocean, and makes new countries; in another, the ocean invades the shores, and makes changes in the shape of continents. In the vegetable world, the leaves are organized in spring, and decay in autumn; and among animals, each has its own period of duration. Among all these organized structures the parts are undergoing unceasing change. Physical forces are at work, physical phenomena have to be originated, and physical ends to be gained. The death, therefore, of an or-

ganic atom in an animal, has for its object the production of a given result, and is itself the result of the action of ordinary physical powers.

20. If, thus, the various movements I am executing in writing these pages arise directly from the respiration of oxygen gas, and its transmission, by arterial blood, through the system, and each letter that I have penned is the result of the death of parts of this animal frame, which, in their removal, have ended in the production of motion and heat, it is plain that there is no essential difference between the death of an organic atom and the extinction of an animal race. Of the thousands of animal forms which have ceased to live, and of which, indeed, we should have no knowledge but for their remains, which have been entombed in the earth, what has been the cause of the disappearance of each successive race? How is it that, at a given epoch, these animated forms have on a sudden stood forth, and after continuing for a time, as suddenly disappeared? Why is it that this disappearance does not take effect in a gradual way, but is so abrupt an affair as to serve, in the hands of geologists, the purpose of marking off one epoch from another? Do not these total disappearances point to external forces of the most extensive operation as the agents that have been at work, and the end and object of those extinctions the production of physical results?

21. As the cause of extinction of those innumerable tribes of life which have inhabited the earth is directly traceable to physical events, for individuals, also, the same law holds good. On the American Continent the mastodon is no longer seen, though but a short time has elapsed since all the rich valleys were thronged with those enormous elephants, and still, in the salt-licks, their bones are disentombed along with those of animals that are here with us. Nor is it alone to obscure and unintelligent orders of life that this law of extinction applies. Within the periods of history, have not the same things happened? The founders of the greatest empires and republics have ceased to exist. Among us, what has become of that ancient people who built the extensive structures in the Western States? Their name and every recollection of them have passed away. Even before our eyes, is not the same thing happening? From the Atlantic States, have not the Indian races nearly disappeared? They are borne by the tide of civilization across the great Valley of the Mississippi. Among them one tribe after another is swept away. The blood of the Mandans has ceased to flow in the veins of a single human being. Worn down by famine, by war, and by pestilence, these children of the forest recede before that civilization, the benefits of which they obstinately refuse to receive, and, clinging with an uncontrollable instinct to a wandering and savage life, accompany the wild animals to the remoter woods, or, driven by their necessities, from time to time return among us, and beg in the City of Washington for a blanket and a little bread.

22. Yet among them there are men capable of the highest emotions and the most noble deeds. The hand of Providence presses upon the Indian. The race, like each individual of it, submits in silence to an irreversible doom. From the day when organization first commenced on the surface of the earth, the law which it has followed has been a law of progress and of evolvement. A myriad types of life have been created, and myriads of living forms produced, and the last is the highest. Even with us the

same thing is going on: advances in knowledge are advances in power. The civilized man of these days is a wholly different being from the man who lived a thousand years ago, and the conditions which determine his position have totally changed. With us the position both of empires and of individuals is fixed by the possession of knowledge—knowledge which is incessantly on the advance. Wherever intelligence has been given, there is a requirement to join in the advancing march. The Indian stands still, and the penalty is death.

23. These severe results are brought about by universal laws—laws which were not intended for individual cases. In the system of the universe an individual is not known, but action takes place on masses. Nor are the laws of Nature ever bent to give benefits to or bring punishment on any individual. They go into effect with an inexorable decision. The earth in her course pursues an irresistible march, and tides rise and fall in the sea with a fixed fatality. In the affairs of men the same unwavering destiny is observed, and whether it be in the case of an empire or a man, resistance to the course of events ends in an inevitable doom. He who resists the progress of civilization, meets the same fate as he who resists the cataract of Niagara. There is no waywardness in Providence, no partialities, and no hates. It is written, "He maketh his sun to shine on the good and the evil, and sendeth his rains on the just and on the unjust."

24. From these considerations, therefore, we may gather that the laws of Nature contain provisions for the extinction and removal of successive races; operations which are carried on by the action of physical powers. As the death of an individual arises from the action of external agents, so, in the same manner, does the disappearance of a tribe: and hence we see that, as existence is under this control, it cannot take place except when physical circumstances conspire; as they change, so, also, must the various forms of life undergo corresponding mutations.

25. In the constitution of all organized beings, water enters as the leading ingredient of their fluid parts; it is therefore obvious that there is a very limited range of temperature in which the processes of life can be carried on. These thermometric limits are between 32° and 212° Fah. Life, therefore, is comprised within a range of 180 degrees.

26. In this manner we might proceed to show how the existence of individuals and races is completely determined by external conditions. How, for the same reason that an individual dies, so too does a tribe become extinct. Pursuing these considerations, we might show how closely the development of the intellect itself is connected with them; we might compare the effect of climates in the torrid, the temperate, and the frigid zone, and show how history bears out the truth of these views. We might appeal to individual experience for the enervating effects of hot climates, or to the common understanding of men as to the great control which atmospheric changes exercise, not only on our intellectual powers, but even on our bodily well-being. It is within a narrow range of climate that great men have been born. In the earth's southern hemisphere, as yet, not one has appeared, and in the northern they come only within certain parallels of latitude. I am not speaking of that class of men who, in all ages and in every country, have risen to an ephemeral elevation, and have sunk again into their na-

tive insignificance, so soon as the causes which have forced them from obscurity cease, but of that other class, of whom God makes but one in a century, and gives him a power of enchantment over his fellows, so that by a word, or even by a look, he can "electrify, and guide, and govern mankind."

27. It was a beautiful idea of some of the English chemists of the seventeenth century, that our earth is nothing more than an incrusted star—a star, the light of which has gone out. They gathered this from the well-known phenomena of hot springs, and the increasing temperature of deep cavities. In their opinion, there was a sun in the earth beneath, as well as a sun in the firmament above. These views are essentially the same as those which are now received by geologists, who almost universally admit the doctrine of a central heat, the surface having cooled down nearly to a condition of equilibrium. Equally beautiful is the idea of M. Poisson, that the sun, in his movements through space, successively carries his attendant planets through regions of variable temperatures—temperatures which are variable by reason of the different amount of stellar radiation which crosses them, and that, for many ages past, he has been coming from a warmer to a colder space. Admitting this to be a true representation of the fact, the phenomena which we witness are such as should take place. If a great mass of rock was brought from the equator suddenly into the polar regions, it would, from being exposed there to a low temperature, commence to radiate its heat, and in a very short time, if examined, would be found coldest on the surface, with a temperature increasing towards its centre, that increase being at first rapid, but the heat subsequently becoming uniform. Such a rock Poisson regards as being a miniature representation of the earth.

28. Which ever of these hypotheses is true, of one thing we are certain, that the surface temperature of the globe has undergone periodic changes, and with these changes, not only has the distribution of plants and animals varied, but general disturbances have taken place in the types of existing species. Those creations and extinctions to which allusion has been made were all, undoubtedly, connected with these thermal disturbances. There can be little doubt that the mastodontoid family was destroyed by a general reduction of temperature. With modifications of the distribution of this all-pervading agent, changes in the distribution of organized beings must of necessity ensue. Even with us, were the mean temperature to rise by a few degrees, the great mammalia of the torrid zone would push their excursions to the north and south. The Bengal tiger would leave his jungle, and press himself into higher latitudes. It would require but a slight meteorological change to bring the orange-tree into the Middle States, or to tempt the turkey-buzzard far to the north of New-York. And, on the other hand, should a reverse action take place, and the mean temperature descend by a few degrees, the indigenous animals and plants of the Northern States must pass to a lower latitude, or become extinct. The distribution of organized forms is, therefore, directly, and their very existence indirectly, determined by the distribution of heat. Who, then, can doubt that all living beings depend on physical force?

29. If these things are true with respect to the organized forms which are known to have existed in former times—and we examine the relations obtaining between the

present climates of the earth's surface and the animals that reside on it—how abundant is the proof that all are still under the control of physical agents! Things being so, and each species having its assigned spot, from which it does not wander, for many ages the same inhabitants are found in the same places. They change as individuals only: the old decline and die away, the young spring up in their stead. I do not know whether, in taking these general reviews of the earth, we ought to have any regard for individuals; whether we ought to consider them as they come into existence and exhibit the strength and vigour of youth, and the decrepitude and languor of old age. Life and death are familiar to us, because as individuals we have a deep personal interest in them; but the cares of Nature are beyond a day. In the Drama of the Universe, each actor performs his part, whether leading or obscure, and, though he may retire from the scenes, the play goes forward to its catastrophe. Whether it be an individual or a race, each, by the actions of its life, has given some turn to the general course of events. In the undulations that circle on a quiet lake, each particle alternately rises up and sinks into repose; but that particle, minute as it was, that motion, small as it might be, was absolutely necessary to keep up the onward motion of the waves. Under this point of view, the destiny of each individual is connected with the destiny of the world.

30. With each breath that I have drawn since I came into the world, I have impressed a change on the earth's atmosphere. One year with another I have removed therefrom eight hundred pounds of oxygen gas, and have cast into it a much greater weight of carbonic acid, made up of the detritus of atoms which have been dismissed from the system as unfit for the continuance of life. It is the express function of the act of respiration to produce this result. That carbon, thus given forth, was received in the form of food, it sated the cravings of hunger, or gratified those animal pleasures which depend on the sense of taste. Taken by appropriate apparatus from the stomach, it passed into the circulation, and, grouped with other materials, it changed into blood; and now, by capillary attraction, aided by the action of the heart, it was carried to every part of the body, and became a constituent of a living being—a temporary constituent only. As we have said, it is a vulgar error that the distinction between a living and a dead body is in the circumstance that the former can maintain itself without change, while the latter undergoes putrefactive decomposition. Both equally putrefy, or, if there be a difference, the change goes on most quickly in a living system. The true distinction rests in this, that a living body is accommodated with machinery to remove the decomposing atoms, and hence the process is conducted in a regulated way. A chemist looks upon life as made up of unceasing deaths. And those atoms of carbon are removed because their life is over, and they pass, by the action of the lungs, into the atmosphere as carbonic acid gas.

31. But their function is not ended. There, it is true, they commingle with the elements of the air, and are no longer fit to support the respiration of man. The ways of Nature are marvellous. This gas, cast away by animal forms, serves now as food for plants and trees. Under the influence of the solar beams, the leaves that have been unfolded in spring, spreading their broad expansions to the air, commence the decompo-

sition of these effete atoms; they undo what the animal system has done; they release the oxygen once more, and restore it to the air for the use of animals, and appropriate the carbon to their own structures. From it they fashion their flowers, from it they build up their stems or perfect their fruit, and prepare once more food for man.

32. Thus do these carbonaceous atoms run through a cycle of change, passing from animals to vegetables, and back again from vegetable to animal systems; and the SUN, to whom Nature has given the charge of these wonderful operations, from age to age furnishes his unfading beams (AP., 824). He sinks below the horizon at night, and retires to the south in the winter, and performs his work by regulated and measured steps. Even at the polar regions, where there is no morning, no noon, no evening, no midnight, where there is no rising nor setting of a star, the great orb peers above the horizon, "and under the influence of his midnight beams, trees and plants run through the same series of changes in a few weeks, which they accomplish in months under the purple skies of beautiful Italy."—BERZELIUS.

33. As, thus, we see the sun performing the office of a great life-giver, and know that all his movements are accomplished by the operation of mechanical laws, as we understand that all these multiplied motions are executed by the action of *one attractive force*, it would seem as though Nature, on the great scale, called on us to recognise the agency of mechanical laws in regulating the processes of life.

34. The carbonaceous matter which has flowed through the heart of man as blood, is transferred, by respiration, to the air, and aids in the formation of forest trees or painted flowers. The Asiatics, with whom have originated all the varieties of pagan creeds that have spread to any extent in the world, believed in a transmigration of souls; they would have been much nearer the truth had they believed in a transmigration of bodies. The coal that we burn is the remains of forests which, in former ages, were thronged with living things—forests that sprang, as do the trees with us, from gases that were formed from the respiration of animals—but of animals that are all extinct.

35. Atmospheric air is, then, the grand receptacle from which all living things spring, and to which they all return. It is the cradle of vegetable, and the coffin of animal life. Made up, as it is, of atoms that have once lived, that have run through innumerable cycles of change, the aspect of purity it presents conceals too well its history. In its ethereal expanse are crowds of atomic forms that have once blossomed as flowers, or participated in the pleasures and pains of animal life. Their former function discharged, these atoms that are dead await their turn of reorganization once more. They occupy themselves in transmitting the many-coloured beams of light, or moving in vibration to the tones of music. A condition so tranquil suits well their former state and future destiny. The remains of wild beasts, or of more ferocious men, disappear in this general tomb of dying atoms, and after a time are reorganized by the solar beams once more, perhaps in those pensive wild flowers that blossom unseen in the gloomy wastes of an American forest.

36. We know that the daily rotation of our earth on her axis determines periodic observances in the functions of organized beings, and fixes their times of activity and sleep; a similar result attends upon her yearly motion in her orbit. How is it that, in our latitudes, trees and plants awake at the coming of spring, and put forth their leaves

and flowers, and then sink again into their annual slumber? How is it, also, that wild birds and beasts conform in their habits to the progress of the seasons, and at one time prepare to bring forth their young, and at another anticipate, with a provident foresight, the coming winter? Those great migrations of fishes that take place at given seasons, and which are even connected with the well-being and wealth of nations, are determined by the occurrence of certain epochs. It is no explanation of these curious facts to say that they depend on other facts like themselves—that an animal sleeps by night, because his prey is also asleep; that a fish migrates at those periods when his instincts tell him that the food on which he lives is abundant. If, in any of these cases, we pass from fact to fact, we uniformly come, at last, to the same conclusion, that all these incidents are directed by astronomical events; that THE SUN not only determines periods of awakening and sleep, of growth and decay, but that there is also committed to him a control and regulation over all the movements of animated beings on the face of the globe. It is the luminous rays of that distant star which, falling perpendicularly, produce the luxuriant vegetation of tropical regions, and his rays of heat which debilitate and enervate the human race. It is, in the polar regions, the obliquity of those beams which suffers the ground to be always covered with snow, and makes those inhospitable countries almost without inhabitants. The trade winds, also, which blow uninterruptedly for ages, carry away towards the poles immense quantities of oxygen gas, which the green parts of plants throw into the atmosphere of the torrid zone. That oxygen is evolved by light, and is then disseminated by heat. In the sea the same influence which thus presides in the air is also at work. The Gulf Stream, which issues from the Mexican waters, with its temperature elevated by solar action. determines the distribution of the Atlantic fishes; the northern whale avoids its offensive warmth, and on its sides shoals congregate which delight in a more genial heat. As it approaches the coasts of Europe, and spreads out into a fan-like form, the vapours that rise from it give forth their latent heat to the air, and moderate the climates of England and France. The coldness and sterility of corresponding latitudes in America is there replaced by a better temperature, and agriculture, the arts of life, science and literature, have there reached their greatest perfection. This physical agent, thus eternally but invisibly continuing its operation, produces a thousand events in which its agency is only remotely traced; nor are those influences limited to mere physical results; they stand in connexion with the progress of society and the evolution of mind. A full development of the reasoning faculty can only take place where physical circumstances conspire. It is to the climate of England and France that the human race is indebted for the intellect of Newton and Laplace.

37. In these remote events, which thus originate among the ordinary phenomena of the natural world, and strike us forcibly when we trace them, step by step, from their origin to their result, we are prone, at a casual glance, to give too much weight to intervening influences, and forget the final cause. Would it be too much to assert that, with returning seasons, periods of vegetation, and the distribution of animated nature, astronomical occurrences likewise direct a thousand of those daily movements which are taking place in every part of the world? There is no harvest which is gathered, no famine which desolates, that has not sprung from an immediate connexion with

them. In judging from a narrow circle of observation, or from an imperfect experience, men are led to regard these as fortuitous affairs. Are they not rather brought about by unfailing and unchangeable causes? From century to century, the sun pours forth his undiminished stores of light and heat; the former, out of inorganic material, constructs molecules that are organized, and from them builds up the myriads of vegetable forms which are destined for supporting animal life; the latter, controlling the movements of inorganic matter, divides into climates the earth's surface, volatilizes water from the sea, sets the wind in motion, and directs the form, duration, and movement of the clouds. The primitive force which is at work, producing these vital and meteorological phenomena, undergoes no change in intensity from year to year; it is therefore expended in producing the same amount of effect. It is through this that the droughts of one country are contemporaneous with the abundant showers of another —the famine which threatens one place is compensated by harvests in another. As natural laws were never meant for operation on individuals, but for action on systems and masses, incidental vicissitudes, such as those of which we are speaking, should never guide us in our judgment of final results. Operating with an unerring certainty, and with an unchangeable force, the sun carries on his plastic works, as the earth, in her daily rotations, submits herself to his beams. From this it comes to pass, that though there may be variations in the lot of particular nations or particular individuals, the common interests of all are protected, the common rights of all are upheld. From the very beginning of things, every class of variation has been determined—where particular climates shall fall, where particular temperatures shall be observed, what shall be the speed of vegetable growth, what tribes of animals shall be given—and the thing remains fixed and unalterable.

38. One of the most striking results of organic chemistry is the relationship which it discovers between animals and plants; the former constituting an apparatus for oxydation, the latter an apparatus for deoxydation. Compared together, a relation of antagonism exists between them. Plants, from inorganic matter, construct their various tissues and parts; these are consumed by animals, and forced back into the inorganic state. It is therefore plain that the sun is the great formative agent, and animals are the destroyers. If we consider the successive races of organized beings, beginning from the lowest and passing to the higher tribes, it would seem as if the general idea under which Nature has been acting is, that as the more complex structures were evolved to emancipate them from the direct control of external physical forces. The vegetable kingdom, unendued with locomotive powers, deriving its existence directly from external agents, is completely under their control. If the summer is too brilliant, or rains do not fall, a plant withers and dies. In the same manner, the lower races of animals have their existence determined by the action of physical causes; if these be favourable, they flourish; if unfavourable, they must submit to an inevitable lot. To tribes that are higher, to a certain extent, the rigour of these laws is remitted, and a certain amount of independence secured; the African lion can retire to a shade in the middle of the day; yet still he is held in a state of subjection, and instinctively submits to the operation of an overruling power, and is kept to the sands of his desert from cool and temperate climates. The sunbeam is his chain. In man alone the emanci-

pation is complete; for into his hand Nature has committed a control of the imponderable principles. It matters not whether it be in the torrid zone or in the frigid, he tempers the seasons by his intellectual power; he resorts to every artifice of clothing, or to the warmth of fire; he dissipates the natural darkness by artificial light. Developed by civilization, he is no longer a prey to natural accidents; if the harvests of his own country have failed him, his hands have created commerce, which brings him an abundance from distant places. Unlike even those races which are next below him, and which instinctively aim at the result he so perfectly accomplishes, he does not wait upon the gifts of Nature, but compels her to minister to him. When they are oppressed by hunger, whole tribes of fishes migrate in the sea, and innumerable flocks of birds direct their flight to distant countries; but civilized man, without calling into action his own locomotive powers, puts his arm across the globe, and satisfies his wants.

39. What, then, are the final impressions left upon our minds by these general considerations? They teach us that life never occurs except in regions to which the imponderable agents can have access, an observation which is equally true of vegetable and of animal forms; that elementary organization directly or indirectly arises from the plastic agency of those all-pervading forces. Whether we consider the organic or inorganic world, all things around us are in incessant changes—changes which result from the fixed operation of invariable laws; that, of the successive tribes of beings which have peopled our earth, each series may be regarded as expressing the general relation of all physical agents at the time of its existence, the brilliancy of the sun, the pressure of the air, and other such conditions; for we see that, between those conditions and the organization of the structures considered, there are fixed relations; that in the more highly complicated forms of beings, mutations more readily take place, and in all, time enters as an element; that in the same way that whole races have disappeared from the face of the earth, and have become extinct, so, also, do individuals die and atoms change; that, whatever motion is accomplished, or whatever change is brought about, there is a consumption of material or an expenditure of force; that, as the surface of the earth is continually remodelled by physical agents, so are the vicissitudes through which organized forms pass determined by physical powers, and bring about physical ends. The passage of a comet, never more to return, in a hyperbolic orbit past the sun, is a result of the same general law that keeps a planet revolving in repeated circles—the extinctions of races which have heretofore taken place, or which are going on before us, are not brought about by a direct intervention of supernumerary forces, but are the constant result of those which are always in action. If, moreover, our thoughts are directed to the relations which exist between climates and the character of races, the distribution of vegetables and animals; if we observe the antagonization of these great classes in the result of their vital processes, their position as respects the atmosphere, the control which astronomical events possess over everything, the action which currents in the air or currents in the sea exercise over the distribution of animated forms, and even over the well-being of man, we surely shall have but little difficulty in understanding that, as in the inorganic world, so also in the world of organization, those all-pervading forces which natural philosophers and chemists recognise are constantly employed.

A TREATISE

ON THE FORCES WHICH PRODUCE

THE ORGANIZATION OF PLANTS.

CHAPTER I.

ON THE ACTION OF THE SUNBEAMS IN PRODUCING ORGANIZED BODIES.

Contents: *The Growth of Confervæ in Water.—Production of Green Matter by Spun Glass and Inorganic Bodies.—It requires a Sporule, Cell, or Objective Germ.—Mode of Diffusion of Gases into Water.—Influence of Temperature on the Process.—Action of the Sun's Rays on these Gases.—Two Atmospheres around the Earth.—Sources of Supply of the expended Gases.*

Application of these Principles to the Production of Chlorophyl in Leaves.—The Digestion of Plants.

40. If we expose some spring water to the sunshine, though it may have been clear and transparent at first, it presently begins to assume a greenish tint, and, after a while, flocks of green matter collect on the sides of the vessel in which it is contained. On these flocks, whenever the sun is shining, bubbles of gas may be seen, which, if collected, prove to be a mixture of oxygen and nitrogen, the proportion of the two being variable. Meantime the green matter rapidly grows, its new parts, as they are developed, being all day long covered with air-bells, which disappear as soon as the sun is set. If these observations be made on a stream of water, the current of which runs slowly, it will be discovered that the green matter serves as food for thousands of aquatic insects, which make their habitations in it. These insects are endued with powers of rapid locomotion, and possess a highly-organized structure; in their turn, they fall a prey to the fishes which frequent such streams. Organic chemistry teaches that it is the office of vegetable life to form from inorganic matter organized molecules, and furnish them as food for the support of animals, which simply assimilate, but do not fabricate; we must, therefore, infer that the fibrine, the albumen, the gelatine, the fat, and whatever else of those compound organic molecules is required for the support of fishes and insects, are originally formed by the action of light on that green matter. But the production of this substance is the result of a multitude of coincident actions. The sunlight is the agent which directs its growth, but it does not so plainly appear what is the body from which it originally springs, and on which the light exerts its influence; whether it comes from microscopic germs which, floating about in the air, find their way into every water, or

from organic cells furnished from obscure sources. The process once begun, goes on with uniformity and rapidity so long as external circumstances are favourable. Some of the chemists of the last century asserted that no organic nucleus was required, and that, on putting such bodies as spun glass or amianthus into a vessel of water, on exposure to light, those fibres would become covered with bubbles, and the water would begin to turn green. But I have found that these are experiments which do not succeed except under circumstances where there is every reason to suspect the incidental introduction of organic matter. Count Rumford has stated that yellow silk, in an unspun state, as it comes from the worm, will cause a very rapid escape of oxygen under the influence of light. It is probable, however, that here, as in the former cases, some specific organic matter has been introduced, for the result very often cannot be obtained.

41. Similar green flocks to those of which we here speak are also found on the surfaces of rocks exposed to the sea, damp walls, and other places where there is a constant moisture. They belong to the algæ or sea-weed tribe of botanists. The confervæ are thus described by Professor Lindley: "They consist of filaments, generally simple, and are formed of two tubes, of which one, which is exterior and transparent, offers no trace of organization to the most powerful eye, so that it might be called a tube of glass, contains an inner articulated filament, filled with colouring water often almost imperceptible, but at other times a very intense green, purple, or yellow colour."

42. Admitting the existence of some objective germ, or sporule, or point on which the light can act, we are able to give a pretty clear account of the process of evolution of the green matter. When pure water is freely exposed to atmospheric air, in consequence of the quality exhibited by all substances (Ap., 75) of diffusing into the interstices of each other, there begin to pass into it the different gaseous bodies which have access to it. These do not all pass with the same facility, nor are they taken up to the same amount. The relative quantity of each follows the order of its solubility. Of the three gases to which our attention must be directed in the phenomenon we are now considering, oxygen, nitrogen, and carbonic acid, carbonic acid passes into the water with the greatest speed, oxygen next, and nitrogen much more slowly (Ap., 54–63). When a state of equilibrium, therefore, has been attained, it will be discovered that the pure water has become contaminated by the presence of these different gases, and that they exist in it to an amount represented by their rate of solubility.

43. Spring water and river water, therefore, naturally contain these different gaseous substances, which are intimately connected with the production of green matter. Indeed, from the very mode in which rivers and springs are fed, the solution of gaseous matter in them is completely secured. By the action of solar heat, vapours are raised from the sea, and ascending to the more elevated and cooler regions, give out their latent heat, and condense into vesicles or microscopic drops, the aggregate of which forms a mist or cloud. In this state of excessive subdivision, the drops are brought into a perfect contact with the air, and an absorption of its constituents takes place. To give still greater security, the drops of rain, which are nothing more than those vesicles coalesced, as they come down from the sky pass through the subjacent aerial strata, and

withdraw as much gas as they can hold. From rains which thus take place and fall on elevated regions, springs and rivers arise, containing, therefore, those gaseous materials which have been obtained from the atmosphere.

44. This water-gas, as it might be termed, may be expelled entirely from water by boiling, and by resorting to that process a knowledge of its constitution has been obtained (Ap., 93). The power by which liquids hold gaseous substances in solution is diminished by heat, and as soon as a liquid boils all extraneous gas is carried away, because, under those circumstances, the vapour which is generated by the heat, passing in bubbles through the mass of the liquid, exposes itself to the contained extraneous gas, which diffuses rapidly into it, and escapes away when the bubbles burst on the top. The law of equilibrium of diffusing gases teaches us that this process can only end in the total removal of the contained air (Ap., 47, 48). Temperature thus controlling the quantity of gas dissolved in water, we can easily see that in different countries the relative amount of water-gas will differ, and that its mean quantity will be connected with the mean annual temperature, and in those seasons during which vegetable action is advancing most rapidly, that is, in spring and summer, its amount will be determined by the mean temperature of the spring and summer months. In tropical climates there should be less of this gas than in temperate, but in the use which is to be made of it for physiological purposes a compensating agency appears; the poorer water of equatorial countries is acted on by a more brilliant ray. The light and heat of the sun here stand in the attitude of antagonizing forces. The calorific beams, by reason of their obliquity in the more polar countries, allow an increased quantity of gas to be dissolved in water; the luminous beams which come with the same obliquity have therefore more to operate on.

45. If saline matter be present, it acts as a disturbing agent; for a liquid which is impregnated with salt has its solvent powers greatly diminished. Bulk for bulk, therefore, sea water must contain a less volume of gas than fresh water.

46. The gas thus included in the interstices of water discharges many important functions. It is connected with the physiological operations both of the animal and vegetable world. Being twice as rich in oxygen as atmospheric air (Ap., 93), it is destined to carry on the respiratory processes of those classes of animals whose form requires that their breathing apparatus should be of the most compendious figure, and which have to live in the water. Thus, in those fishes which breathe by branchiæ, the processes of combustion which are to go on in their systems depend on a supply of the concentrated oxygen of water-gas. The fish swallows a mouthful of water, and then, by muscular contraction, drives it out past the gills, among the threadlike fibrillæ in which venous blood is passing. Inter-transfusion in an instant takes place, oxygen from the water-gas flashes into the venous blood and arterializes it, carbonic acid simultaneously comes out, and, as the fish moves forward, is carried away in the current of water.

47. A mass of water thus containing carbonic acid, oxygen, and nitrogen, with some germ, or sporule, or objective point, on which the light can act, is exposed to the sun. As has been said, a bubble of gas soon makes its appearance, and growth, with a devel-

opment of the green colour, takes place. If we examine the changes which are now occurring in the water, we find that the carbonic acid is disappearing, and oxygen and nitrogen are evolving. The growing mass increases in volume and weight. After a time, if proper measures are taken to cut off any farther supply of carbonic acid, the process comes to an end. But as conducted naturally, there is a free exposure to the atmosphere, from which the acid diffuses, and hence, as fast as its removal by decomposition takes place, new quantities are restored again.

48. The direct action which is accomplished under the influence of the light is the total decomposition of the carbonic acid gas. Its oxygen is evolved, and its carbon goes to form the green matter. At first sight, the supposition of the generality of chemical writers seems to be probable, that the carbon thus evolved, while it is yet in a nascent state, unites directly with the elements of water, and produces one of the starch family of bodies. But the constitution of the green matter is far from being so simple. Chemical examination, and considerations connected with the nutrition of the insects which feed on it, serve to show that it contains more hydrogen than is necessary to convert its oxygen into water. There is also nitrogen in it.

49. In the air-bubbles which form and finally escape, we find not pure oxygen, as is generally supposed, but a mixture of oxygen and nitrogen gases. The source of the former is unquestionably to be referred to the decomposed carbonic acid. From the circumstance that in the growing mass there is an excess of hydrogen, and in the escaping air a large quantity of azote, it would seem that a compound having the same elements as ammonia has been acted upon. Nevertheless, the quantity of ammonia which is commonly dissolved in spring or river water is far too small to furnish the supply which would be necessary. It seems, therefore, not improbable that the nitrogen naturally dissolved along with the carbonic acid and oxygen in the water is indirectly connected with the decomposition.

50. The presence of carbon, and an excess of hydrogen in the green matter, proves that the sunshine has effected two different decompositions, the decomposition of carbonic acid and that of water.

51. At this stage of the description, let us pause and review the facts which have presented themselves to us. We have arrived at the conclusion that all the solid material under consideration is produced from gaseous matter contained in water in a state of solution. This gaseous matter comes directly from the atmosphere; it contains the same constituents as the atmosphere, but differs from it in having them in a different proportion. We see, therefore, that there are, as it were, two atmospheres enveloping the earth's surface; one of these is the air which we breathe, the other that which exists dissolved in the sea, and in those streams and waters that are found on the surface of the ground. Between these a remarkable relation exists: in the common atmosphere the proportion of oxygen to nitrogen is nearly as one to four, in the water-atmosphere it is nearly as one to two.

52. Between these two atmospheres, a constant connexion is established, through the chemical relations of water, which receives its gaseous contents from several different sources. Part of its carbonic acid it obtains by direct absorption from the superincumbent air, and part is given to it by the respiration of fishes and other aquatic ani-

mals; for animal life, though carried on beneath the surface of this liquid medium, has the same chemical object in view as when carried forward on the surface of the earth; it ends in the conversion of carbon into carbonic acid, of hydrogen into water, and an evolution of heat. Its oxygen, in the same manner, has two sources of supply, direct absorption from the air, as in the former instance, and a still more concentrated store in those little air-bells that cover the green vegetable matter as long as the sun is shining; in these the volume of oxygen is, on an average, double that of the nitrogen. Upon them the water exerts its solvent powers, removes as much as it can carry away, and the bubble then floats to the top of the water, and escapes out into the air. From the same two sources nitrogen is also procured. With these abundant supplies, therefore, furnished partly from the vital changes which are taking place in its mass, and partly from the external air, the constitution of the water-atmosphere is kept up unimpaired.

53. All the carbon which we thus find in the green matter comes from dissolved carbonic acid, all the hydrogen from water or ammonia; the latter also furnishes a certain quantity of nitrogen, other portions of which are obtained, without any decomposition, from that which has been dissolved out of the atmosphere. The general principles of life carried on in the water are modelled on the same idea as in the case of life carried on in the air. In both cases, vegetables act as the great formative agents, and animals as the destructive power; and in both, the source and origin of action is to be found in the beams of the sun. For nearly two centuries, physical science has fully admitted the agency of that central star as the great seat of mechanical force, which retains the different planets in their orbits. It is only of late years that we are beginning to recognise his agency as the author of organization and life, who lays up, with an almost provident foresight, in vegetable productions, stores of light and heat for the use of the animal world. The coal-fields which furnish us with fuel are the remains of primeval forests, among the branches of which birds nestled at night; and the warmth that we receive from them, and the light that they give us, have been safely stored up for us for thousands of centuries. Those little insects, also, which at certain seasons cause the sea to shine with a phosphorescent light, derive their glow remotely from the vegetable kingdom; and the fireflies, which, in such countless multitudes, on a summer evening in Virginia, make the grass and trees glitter with their intermitting beams, are only pouring forth again rays which once came from the sun.

54. I have thus far considered the process of evolving green matter from a primitive cell or seed placed under water. It is now proper to generalize on these views, and to show that, so far from this being an insignificant case, it represents fully all that goes on in the vegetable tribes, whether they live under the water or in the air. Let us, therefore, proceed at once to investigate what takes place in the case of plants which are of a more complex character, and higher in the scale of creation. If a few garden seeds of any kind are sown in a flower-pot, and caused to germinate in a dark room, after a while it will be perceived that they can grow for a certain space in the absence of light; their young leaves, if any should be put forth, are of a yellow or gray-white colour, and they soon fade away and die. But if these plants be brought out into the light, they

presently begin to turn green, they unfold their leaves, and evolve their different parts in a natural way. From day to day their weight increases, and chemical analysis shows that they are fixing carbon, hydrogen, oxygen, and azote. If they be made to grow in confined glass vessels, under such circumstances that an examination can be instituted on the changes they are impressing on the atmosphere, it is discovered that they are constantly abstracting carbonic acid from it, and as long as the sun shines on them, or as long as they are exposed to bright daylight, they continue appropriating carbon and exhaling a mixture of oxygen and nitrogen. The continuance of their growth depends on a continued supply of the acid gas in due quantities. The leading facts which are here mentioned were discovered during the last century by Priestley, who found that when leaves of any kind are placed in water, which holds carbonic acid gas in solution, they evolve oxygen when in the sunshine. It is not pure oxygen, but a mixture of that gas with azote (Ap., 794). It has been objected that no conclusion can be drawn from experiments conducted in this way in regard to vegetable functions, for a plant which is fitted to carry on its living processes in the atmosphere is placed in an unnatural condition when immersed in a vessel of water. But there is much less force in this objection than might at first sight appear. A leaf, exposed to the air, does not absorb and act upon carbonic acid gas as a gas; its tissues and parts are saturated with water, which has been thrown up by the capillary force of the spongioles, or, on some occasions, obtained directly from the atmosphere by the beautiful process of nightly radiation to the sky, and corresponding precipitation of drops of dew; this water, thus penetrating every part of the succulent structure, is the medium through which carbonic acid is absorbed and decomposed. In point of fact, therefore, though plants may live in air, their mode of discharging this function is the same as though they were living in water, for the absorptive force of that liquid is called into play, and carbonic acid is presented in a dissolved state, and the case, in reality, becomes nothing more than a repetition of what goes on when water-plants are digesting. In very many instances Nature takes extraordinary pains to secure a rapid supply of the acid. In water-plants, which are often fixed, reliance is had on currents which are established by variations of specific gravity, so as fast as one portion of water has approached to the place of decomposition and surrendered its gas, it is pressed away by those around, which are about to discharge the same duty. But in aerial plants, the digestive organ unfolds a broad surface to the atmosphere, and the calorific beams of the sun descending on it, by reason of its dark colour are readily absorbed; a warm current, which is easily established in a mobile fluid like air, rises rapidly; and, as if this were not enough in multitudes of instances, the trembling leaves are set upon slender footstalks, which give way to every passing wind, and are thus continually brought into an extensive and ever-changing gaseous contact. It is scarcely possible to conceive a more simple and effective contrivance, or one which reaches more perfectly its destined end. To animals powers of locomotion are given, with a view of securing a possession of food; at proper seasons the wild pigeon comes up from the southern countries, and instinctively flies thousands of miles; and in the case of many beasts, to procure their prey seems to be a principal cause of movement. An animal, an oxydating machine, is driven by Nature to expend his powers of loco-

motion, and go in search of his food; a plant, a reducing apparatus, has a more delicate duty to perform, and Nature herself becomes its minister, and offers it a supply for all its wants.

55. Under these circumstances, therefore, when leaves of aerial plants are placed in carbonated water in sunshine, we can, so long as their structure remains unimpaired, observe with a certain degree of correctness the phenomena which they would exhibit under more natural conditions. By botanists and the earlier writers on chemistry, the function discharged by the green parts of plants is often spoken of as a species of respiration, analogous, to a certain extent, to animal respiration. This mistake originated in those obscure physiological views of which the old doctrine of vitality was the prolific parent. Respiration is essentially an oxydizing process, a process of combustion, but the part which is played by a vegetable leaf is to obtain carbonaceous matter from air, and store it up in a solid form in its various strictures. This solid matter thus obtained is the very same which at an after period, entering the digestive organs of animals, is by them subjected to assimilatory processes, and finally becomes a part of their fabric. The action which is performed by those green parts is not, then, a respiratory action, but one of digestion.

56. We have already said that leaves placed in spring water in sunshine evolve bubbles of gas. A direct sunshine is not, however, absolutely required; the same experiment, if conducted with proper precautions, can be made to succeed with the diffused skylight. The bubbles, as they form, rise to the top of the water, and if collected and analyzed, yield, as in the case of water-plants, two substances, oxygen and nitrogen; a little carbonic acid is always present, but that arises from the peculiar conditions under which the experiment is made (Ap., 794).

57. Thus, by the influence of the sunlight, organic matter is added to vegetable systems, the action being accompanied by a variety of chemical decompositions and interstitial diffusions. The substances arising are such as are necessary for the uses of the plant, and in order to distribute them to the requisite parts, mechanical motion has to take place. This, in the more highly organized plants, goes under the designation of the flow of the sap. The descending sap, like the arterial blood of animals, contains all the different compounds which are required by the organized structure. We shall in the next chapter consider the causes which direct the movements of this liquid.

CHAPTER II.

ON THE MECHANICAL CAUSE OF THE FLOW OF THE SAP IN PLANTS. IT IS DUE TO THE CARBONIZATION OF WATER IN THE LEAVES BY THE LIGHT OF THE SUN.

CONTENTS: *The Flow of Sap and Circulation of Blood are probably due to the same Physical Cause.—Amount of Water circulating in Plants.—Botanical Theory of the Flow of Sap—fails for the descending Sap.*

Capillary Attraction described.—Elevation or Depression of Liquids depends on their wetting or not wetting the Tube.—No Flow in an ordinary Capillary Tube.—Conditions for producing a Flow—such as Evaporation, Decomposition, and Solution.—Endosmosis produced on these Principles by Solution.—Dutrochet's Experiments.—Explanation of them.—General Law of these Movements.—Force with which they take place.—Capillary Attraction due to Electricity.

Application of these Principles to the Ascent of the Sap.—Exhausting Action of the Leaves.—Cause of the Descent of the Sap.

The Light of the Sun is the Cause of the Flow of the Sap both in its Ascent and Descent.

58. IN the lower classes of plants, such as those of which we have been speaking, which carry forward absorbent processes on every part of their surface, the mechanism for nutrition and respiration is of the simplest character, and, as we shall hereafter see, is nothing more than a surface action; imbibition, nutrition, and aeration all taking place upon the same point. But in any organized structure, as soon as types of centralization are adopted, and specific processes carried on in distant parts, the nutritious juice must necessarily pass from place to place, and undergo in its route proper chemical changes. In its movements it must flow along predetermined channels, and take in succession given directions. To accomplish this a circulatory apparatus is required, adapted in each instance, as to form and character, to each peculiar organism. In the higher classes of animal life, this process of movement passes under the designation of the circulation of the blood; in plants, under the designation of the flow of the sap.

59. There is that unity of plan in all the works of Nature which causes us at once to understand that in these various mechanisms the same physical principles are resorted to; that the flow of the sap and the circulation of the blood are due to the same powers. A theory of such movements, therefore, can only be true when the principles which it involves give at once a clear account of every case, of the flow in plants and in animals, and even in every individual instance in each of these great classes of organized beings. Such a theory should be applicable to the movements in flowering plants, and to all its various modifications in the less complicated orders of vegetable life; for each particular instance it should show why specific apparatus is required, and

why, in cryptogamous plants, none whatever is necessary; it should show, among the varieties of animal races, why this or that mechanism is employed; why insects have no heart, or why in fishes the aorta resembles in mechanism the portal vein of the mammalia. In those more elevated tribes whose functions require that several circulations should be simultaneously carried on, it should give a clear account of the mechanism for them, whether they be systemic, pulmonary, or portal. The character of a true theory is its extensive and clear application to all individual cases.

60. In this and the following chapter, I propose to explain what appear to be the true mechanical principles of the general circulation of organized beings. For some years past I have taught these doctrines to the medical classes of this University, though until now they have not appeared in print. Founded as they are on principles strictly physical, they form an important portion of the views set forth in this work. If among plants light is the great agent of organization, electricity is the great motive force, which, under a specific modification, determines the movements of nutritious juices. The imponderable agents are the vital principle of organized systems.

61. It will be perceived that the doctrines here given apply to all cases; they are also in harmony with physiological knowledge. They embrace not only the vegetable and animal world, but the specific instances in each. In individual cases, they give an account of the mechanism of the various circulations, whether they be systemic, pulmonary, or portal. They also will be found to apply to those abnormal cases which arise in disease, and offer an exact prediction of what should take place in inflammation and asphyxia.

62. First, therefore, let us take up the consideration of the circulation of the sap in flowering plants, and show the physical conditions on which it depends. In the following chapter we shall apply the same principles to the circulation of the blood.

63. Early in the spring, in the Southern States, the farmers plant their corn—the *Zea mays*. In the course of some days, if the weather is warm and favourable, the seed begins to germinate, and presently the young plant appears above the soil. As soon as this has taken place, the shoot turns green, the radicles begin to extend themselves, and growth rapidly sets in. In a few weeks the plant has risen to an altitude of several feet, and exposes its large green leaves to the sun. By the end of July, or the middle of August, its maximum height is attained, and, under favourable circumstances of culture and season, so great is that height, that a man on horseback cannot touch the tassel which adorns the top of the plant with a walking-stick. All this enormous evolution of organized structure has, in the course of a few days or weeks, originated from a seed buried in the ground—a seed which weighed but a few grains.

64. From the roots to the top of the plant, large quantities of fluid are constantly passing, and large quantities are thrown off from the leaves by evaporation. All this water is obtained entirely from the soil, and all the carbonaceous matter which constitutes the solid part is derived from the atmosphere; and, inasmuch as even the water itself descended originally from the air in showers of rain or drops of dew, we are justified in saying that this immense mass of organized matter is nothing more than a portion of the atmosphere, which has been condensed and fashioned in a few weeks by the agency of the imponderable principles.

65. In those hot climates, the quantity of sap which flows in a short space of time through the vessels of such a plant is incredibly great. In the month of April, 1834, I cut a vine, which was growing wild on the edge of a forest in Virginia, asunder with one blow of an axe; the cut surface, which was about $1\frac{1}{2}$ inch in diameter, exhibited its open vessels, from which there poured out an uninterrupted stream of ascending sap. In the course of eight hours there was collected of this fluid seventy ounces, and this was probably a far less quantity than would have been raised under ordinary circumstances, where the leaves aided the spongioles by their exhausting and pushing action, in the way which will be presently explained. As all persons know, in these warm climates processes of vegetation go on with extraordinary rapidity, and in summer, from the intense brilliancy of the sun and the high temperature, the midday assumes peculiarities which in colder climates are never witnessed. In the forests of those countries, at that hour, there reigns an unbroken silence, a period of complete repose. The wild dove, which all morning long has poured forth her plaintive note from the top of some withered pine, seeks for a shady covert, and plumes her feathers in the heat of the day; the wanton squirrel forsakes his gambols, and retires to his nest; the turtle dozes on the surface of the stagnant pools. There is not a cloud upon the sky, there is not a breath in the air. The sunbeams tremble upon the leaves, or sparkle upon the sand, or steal in long gleams of light across the water. A profound silence reigns among myriads of living things, inhabitants of those solitudes; a silence only broken at intervals by the rustling of the brown lizard in the leaves, or the distant tap of the lazy red-headed woodpecker upon some hollow trunk.

66. As we have said, if at the proper season of the year the stem of a plant be divided, ascending sap will copiously flow from the extremity of the stump. If, moreover, the part which has been cut off, and which bears the leaves, has its cut extremity immersed in a vessel of water, imbibition of that water will rapidly take place, and life be maintained for a time. From these facts, and others of a similar kind, botanists have had no difficulty in recognising two distinct sources of action concerned in the flow of the sap. They have shown that the spongioles or extremity of the roots, which apparently consist of a lax cellular or spongy tissue, have the quality of impelling the sap upward, and hence it flows from the extremity of a stem from which the upper part has been cut off. From the fact that a branch on which the leaves still remain possesses the quality of removing water from a vessel in which its extremity is immersed, they have regarded those organs as possessing a kind of suction power, due to the evaporation which takes place under the influence of heat from their superficies. The spongioles, therefore, drive the sap upward, and the leaves, by their exhaustive effort, draw it.

67. But this doctrine is essentially defective. It furnishes no reason for the downward flow of the sap; none for the well-known fact that the exhausting action of the leaves is under the control of light. To a certain extent, it is true, an evaporation taking place from the leaves will conspire in its result with the movement of the ascending sap, but it must not be forgotten that it will also antagonize with the movement of that which is descending. That there are two seats of action in the phenomenon, the spongiole and the leaf, is quite true; but its causes a e very different from those gener-

ally supposed. The action in the leaf and the action in the spongiole is the same, and it is precisely an example of what takes place in the passage of arterial blood to the veins in the systemic circulation of the mammalia.

68. It is not worth while to expend any space here in refuting the old explanations of these different circulatory movements, nor in detailing how, in the opinion of some physiologists, even in the most rigid vegetable stems, the juices are propelled by alternate dilatations and contractions of the woody tubes, much in the same manner as motions are executed by the dorsal vessels of certain animals. Nor need we describe how the vital principle—that chimera of the Dark Ages, which has kept physiology in the rear of all other sciences—can be brought to give one of its usual, and expeditious, and unsatisfactory accounts of the phenomenon.

69. Without wasting time, therefore, on those futile explanations, let us pass at once to the philosophical principles which are involved, and show how the doctrines of common CAPILLARY ATTRACTION are capable of giving not only a comprehensive, but also a beautifully simple explanation of the whole phenomena, no matter whether they are found in flowering or in flowerless plants, in sponges, or in the mammalia.

70. CAPILLARY ATTRACTION, of the physical cause of which I shall presently speak, takes its name from the circumstance, that if a glass tube of small diameter, or even as fine as a hair (*capillus*), be immersed at one end in water, the water immediately rises above its true hydrostatic level to an altitude which is greater in proportion as the tube is smaller; in tubes of very narrow diameter, such as those here referred to, an elevation of many inches is without any kind of difficulty obtained. Thus, in *fig.* 108, if some water be placed in a cup or other vessel to the height A B, and there be plunged into this water glass tubes such as D, E, the water at once spontaneously rises in those tubes to a height which is greater in proportion as the tube is narrower. In E, therefore, it rises higher than in D.

71. But this elevation from the true hydrostatic level only take place with certain liquids in certain tubes. Thus, with the same glass tubes, D and E, if quicksilver is used instead of water, so far from there being an elevation, there is an analogous depression. The liquid metal is forced down, as it were, beneath its proper level to a greater depth in proportion as the tube is narrower. And that this depends on the chemical relation which subsists between the liquid employed and the substance of which the tube consists, is clearly shown by smearing the interior of a glass tube with tallow or oil, and then immersing its end in water. The water, under these circumstances, so far from rising, is, like quicksilver, depressed.

72. The physical law under which these elevations and depressions takes place is very simple, and important to be remembered. If a liquid can wet the surface of a solid, it will rise in a tube formed of that substance; but if a liquid cannot wet a solid, it will be depressed below its true level in a tube formed of that substance.

73. Suppose, now, we had a glass tube immersed in water, a tube of such diameter that it could cause the water to rise to the altitude of twelve inches, and the tube be broken off so that it is only six inches long. The theory of capillary attraction, and also direct experiment, show that under these circumstances the water will rise to the

top of the tube, *but will not flow over*, as one might perhaps have expected. In an ordinary capillary tube, therefore, nothing like a constant flow can take place, but the liquid having attained its highest possible elevation, remains there.

74. That a continuous flow should take place, all that is necessary is, by any proper means, either by evaporation, chemical action, or other processes, to remove away the superficial portions of the elevated liquid when they stand at the extremity of the tube. An illustration will show how this is accomplished. The wick of a lamp is nothing more than an extensive system of capillary tubes, tubes which are formed by the juxtaposition of the cotton fibres. As common experience satisfies us, such a wick may be immersed in a reservoir of oil for months, or even years, without any sensible portion of that liquid being removed; but if the lamp is lighted, the process of combustion dissipating the oil as fast as it reaches the upper portion of the wick, fresh quantities are furnished from beneath, and a continual flow takes place until all the oil is gone. So also in a spirit lamp, as long as the extinguisher is over the wick, and no evaporation of the alcohol can take place, there is no flow; but the moment the extinguisher is removed, so that evaporation into the atmosphere can be accomplished, there is a constant upward flow until the alcohol is dissipated. These are results which have been long recognised.

75. From these elementary considerations, therefore, it is clear, that although, in the ordinary use of a capillary tube, continuous movement along it is not witnessed, that movement readily sets in as soon as the proper conditions are fulfilled. In the two cases we have used as illustrations, combustion in the one, and evaporation in the other, gave rise to a continuous motion.

76. In the same way a variety of other ordinary causes may produce these movements. Suppose, *fig.* 109, we had a vessel A, containing water, and another vessel B, containing alcohol, and between them a narrow capillary tube, C, passing. Let us farther suppose this tube C to be previously filled with water. At its extremity which opens into the vessel B, containing alcohol, it is clear that the water will be brought in contact with the alcohol, but in this liquid water is soluble; and, therefore, as fast as that water can be presented, the alcohol will dissolve it, take it up, and remove it away. It is plain, therefore, that there will be a constant flow of water from the vessel A to the vessel B, a flow which arises from the circumstance that the water is dissolved from the end of the tube as fast as it presents itself, by the affinity of the alcohol for it. This, therefore, is nothing more than a repetition, under another form, of the cases already used above (74) as illustrations.

76. But an attentive consideration of the facts will convince us that the affinity thus existing between the two liquids, and which thus gives rise to a flow from A to B, ought, after a short time, to cause portions of the alcohol to find their way through the water in the tube C, and present themselves in a certain quantity at its entrance into the vessel A; there, in their turn, they are exposed at once to the pure water, which takes up and dissolves alcohol just in the same way that alcohol takes up and dissolves it. There must, therefore, be a constant flow of alcohol, from B to A, along the tube, for the very same reason that there is a simultaneously constant flow of water from A to B, in the contrary direction.

77. The general result of these considerations proves, that if two different liquids, which can dissolve one another, communicate through a tube which both can wet, both of them will flow through that tube contemporaneously, the one passing in one, and the other in the opposite direction; and it is plain that all this is nothing more than a simple case of common capillary attraction.

78. What happens through one will, under similar circumstances, happen through one hundred or any number of tubes. If, therefore, instead of a single tube C, a great number of tubes were made to communicate between A and B, they would all act alike, and through them the two liquids would simultaneously pass in opposite directions.

79. It is also obvious that the shorter we make the tube C, or the supposed collection of tubes, the more readily will the flow take place, because the vessels A and B are then made to communicate through a shorter obstacle. If, therefore, we take a box, A B, *fig.* 110, and divide it water-tight by any porous obstacle C, such as a piece of paper, or bladder, or porous earthenware, &c., which substances may be regarded as consisting of a congeries of very short tubes, their pores answering to such short tubes, and in the compartment A place water, and in B alcohol, through the intervening barrier interchange will take place; and if there be no leakage, and one liquid passes more rapidly in its course than the other, not only will the interchange we have been describing take place, but there will be also an accumulation of liquid on one side of the barrier C, and a diminution of it on the other.

80. All this is irrespective of the shape or form of the vessels, which may be cubical, or round, or of any other figure. The essential conditions for action are to have two liquids, which have an affinity for one another, placed on opposite sides of a pervious obstacle or porous barrier, which both of them can wet. Movement will then take place in opposite directions; and, if one liquid flows more rapidly than the other, there will be an accumulation on that side of the barrier to which it goes, and a deficiency on the other.

81. M. Dutrochet took a bladder, and, filling it with alcohol, tied the mouth of it tightly, so that none of the liquid could escape. He then placed it in a vessel of water, and found that the alcohol came out of the bladder into the water, and the water passed through the bladder into the alcohol, and, inasmuch as the water flowed more rapidly than the alcohol, there was a constant accumulation within the bladder, distending it; an accumulation taking place with sufficient force to burst it open, provided the experiment was continued long enough.

82. To these phenomena M. Dutrochet gave the name of Endosmose and Exosmose, the former in allusion to the current flowing inward, the latter to that flowing outward. Physiologists were pleased with these sonorous designations, which ever since have been used in the books. Some supposed that the force thus exerted by a bladder was due to the remains of vitality still existing in it, arising from its organization. It is, however, as we have seen, one of the ordinary cases of common capillary attraction, with which the vital force has no more connexion than its kindred principle Phlogiston.

83. The rise or depression of a liquid in a capillary tube is determined by its quali-

ty of wetting or not wetting the surface of that tube. Of two liquids in a given tube, that will rise highest which will wet the tube most perfectly. And, therefore, we can see in these movements, that if two liquids be placed on opposite sides of a porous system, or at the opposite ends of a capillary tube, which is wetted by one more perfectly than by the other, that one which exerts the most energetic action will flow fastest. If a piece of bladder be soaked in water and in alcohol, it will be readily seen that the former acts more powerfully on it, giving it greater flexibility and translucency, and having a stronger affinity for its tissues. For these reasons, if a mixture of water and alcohol be tied up in a bladder, as is well known, the water will soak out and evaporate away, but the alcohol will be retained. And for the very same reason, when these two liquids are placed on opposite sides of such a porous body, the water moves fastest through it, as in the experiment of DUTROCHET (81).

84. From these simple principles we deduce the following important law—important, because it gives us at once a clear explanation of the rise of sap in trees, and a beautiful exposition of the true cause of the circulation of the blood: *When two different liquids are brought in contact in a porous solid, which is wetted by both, but by them unequally, that one which has the greatest affinity for the solid, or which wets it most perfectly, will pass most rapidly through it, and may even drive the other entirely before it.*

85. This passage is not accomplished with an insignificant force. Direct experiment shows that (AP., 145) water will thus pass into alcohol through a pervious membrane with a force equal to the pressure of nearly two atmospheres. Nor is it alone between liquids that the phenomenon takes place; it is exhibited also by gases. Here, again, the force with which the movement has been accomplished is surprisingly great. Sulphurous acid will pass (AP., 158) into atmospheric air against a pressure of one hundred and ten pounds on the square inch, and sulphuretted hydrogen will move through a membrane with a force that is superior to a pressure of twenty-four atmospheres (AP., 162). As a mechanical agent, capillary attraction, therefore, is fully able to overcome any of the resistances which it has to encounter in elevating sap to the tops of the loftiest trees, or driving blood from the remotest parts of the largest animals.

86. In the Appendix I have given (CHAP. V.) an account of the physical principles on which capillary attraction itself depends. It is needful, therefore, in this place, only to sum up the evidence there brought forward, that it is nothing more than a manifestation of electricity, and that all capillary phenomena are cases of electrical attraction. If we take a piece of flat glass, and place it on the surface of some mercury, the glass is held to the metal with a considerable degree of force, so that it requires some exertion to separate them. When this is done, and the electric condition of the mercury and glass respectively examined, the one is found to be positively and the other negatively electrified. They must therefore attract each other (AP., 116); and it is owing to this electric excitement, which always takes place upon the contact of bodies, that all the phenomena of capillary attraction are due. Thus, if two pieces of plate glass are brought in contact, they are found to cohere, because the one is positive and the other negative,

as may be proved by examining them with an electrometer. Even in those cases in which attractive forces are developed without an apparent electric disturbance, these principles apply. Two leaden bullets, which have been brought (AP., 126) into contact with one another, cohere strongly, but we are not able to show the development of the separate electricities on each, because of their high conducting power. So, too, when a piece of glass is laid on the surface of some water, and, on being lifted off, is carried to the electroscope, no development can be detected, and the reason is obvious, for there has been only an apparent, and not a true separation of the liquid and the glass from each other; the particles of the liquid have been simply torn apart, and have not been separated from the glass.

87. Referring, therefore, to that chapter for the proof that capillary attraction originates in electric disturbance, it is sufficient for our present purpose to remark, that it is therefore due to the very same cause as chemical affinity itself. The quality which liquids possess of wetting or not wetting the surface of solid bodies is, therefore, nothing more than an indication of the affinity which is between them. Quicksilver will not wet glass, because they have little affinity, but it will wet a surface of gold or of tin with facility, because its affinity for those bodies is energetic. These observations, which appear so simple, have very important applications; the intensity of affinity between a given liquid and a solid with which it is brought in contact determines their capillary relations, and thereby determines, also, the phenomena of movement. No farther proof of this importance is, perhaps, required than the result to which we shall be presently led, *that even in* MAN *the circulation of the blood is caused by the oxydating action of that liquid on the solid structures with which it is brought in contact.*

88. Let us, therefore, finally remember that the explanation of the circulation of nutritious juices, both in the vegetable and the animal kingdom, rests upon this simple physical principle, *that if two liquids communicate with one another in a capillary tube, or in a porous or parenchymatous structure, and have for that tube or structure different chemical affinities, movement will ensue; that liquid which has the most energetic affinity will move with the greatest velocity, and may even drive the other fluid entirely before it; that this is due to common capillary attraction, which, in its turn, is due to electric excitement.*

89. These things being understood, let us proceed now to apply our principle (84) to the cases in hand, and commence with giving the theory of the flow of sap in plants.

90. The liquid of which the ascending sap is constituted is derived from the ground by the action of the spongioles, and consists of water holding in solution the different saline bodies which are necessary to the plant, along with carbonic acid, &c. This compound fluid passes upward by the woody fibre and ducts of the alburnum; making its way to the leaf, on the upper surface of which, in common cases, a change in its chemical constitution occurs through the influence of the sunlight. It obtains a certain quantity of carbon, and from being a thin watery solution, becomes much concentrated, and gains the under face of the leaf. This elaborated sap, or *latex*, as it is frequently called, returns now to the bark, and descends through its cellular tissue and inter-cellular spaces, finding its way by the route of the medullary rays to all parts of

the plant. During its descent the different vegetable principles necessary for the economy of the plant are removed from it, and a certain quantity goes down to the roots, partly to aid in their growth, and partly to throw new qualities of ascending sap into the tree. In this descent, the elaborated sap moves through a system of vessels which anastomose with one another, in the same manner as the capillary vessels of animals. These tubes go under the technical name of laticiferous tubes.

91. We see, therefore, from this description, that there are two points of this circulation which require attentive consideration—the spongiole and the leaf. The spongioles are nothing but the young succulent extremities of the roots, which have been recently formed from portions of the descending sap, but that sap is itself a species of mucilaginous solution. Precisely, therefore, as water will pass through the tissue of a bladder, the interior of which is filled with gum-water, so will moisture from the ground flow through the spongiole. There is no difficulty in accounting for the rise of the ascending sap on the principles of capillary attraction, and, indeed, this is the explanation now generally received by vegetable physiologists.

92. While, therefore, those philosophers have freely admitted the applicability of this principle, under the name of endosmosis, to the explanation of the ascent of the sap, they have attributed to it, as an aid, a force which comes into operation only in an incidental way. This is the exhausting action of the leaf. But it is probable that this force takes little or no part in the movement of the sap, for any tendency to a vacuum occurring in the leaves of a tree would cause those structures to collapse, and not eventuate in exerting suction power on the rising fluid. It is true, that if we take (67) a branch covered with its foliage, and dip its cut extremity into water, imbibition of that water will rapidly take place; but the phenomenon is certainly not due, as is ordinarily supposed by botanists, to the exhausting action of the leaf, but to a very different cause. The exhaustion is only an incidental affair.

93. Guided now by the principle we have laid down (88), let us predict what must be the necessary action of the leaf. The ascending sap, which we will suppose, for simplicity's sake, to be water, rises to the upper face of the leaf. It there obtains carbonic acid gas from the air, of which the sunlight effects the decomposition, the resulting action being a change from water to a mucilaginous solution. In the tissue of the leaf we have, therefore, two fluids engaged, water and a mucilaginous solution; and what must of necessity be the result? The water will drive the mucilaginous solution before it (84–88), and force it back, along its proper vessels, into the stem. The imbibition, therefore, that we perceived when a branch is dipped in a vessel of water, does not arise, as the botanists say, from evaporation taking place on the leaf, but it comes from the capillary reaction which is going on in the leaf between the water, which is then presented as ascending sap, and the mucilaginous solution which has been formed by the light of the sun. Evaporation, it is true, takes place, and comes into operation, as I have said, in an incidental way, but the proper force which gives origin to the whole phenomenon is the capillary action which is going on in the way just described.

94. It is the imperfection of the principle on which they were relying—the exhausting action of the leaf—that has caused botanists to look upon the descent of the sap

as such a mysterious affair, for which they could offer no explanation. Physiologists have here stepped in with their phantom vitality, and have explained the descent of the elaborated sap on visionary hypotheses, that it was alive, or had obtained some vital qualities. It had long been perceived that gravitation could have little or nothing to do with the motion, for the descending sap flows upward in a dependent branch.

95. What, then, is the reason that the light of the sun controls the rapidity of imbibition—the speed with which the ascending current comes? Because it controls the amount of carbonic acid gas which is reduced, and, therefore, the amount of elaborated sap that is formed. Why is it that the flow from the roots diminishes when changes are befalling the leaves, and why does it stop in the winter? Because the mucilaginous solution which is made by the light diminishes in quantity, or ceases to be formed altogether. How is it that, when parts are rapidly developing, the latex moves fastest, and the ascending sap comes with most force? Because the consumption and the consequent formation of the mucilaginous matter are then at a maximum.

96. We see, therefore, that the two sources of force in a flowering plant, the spongiole and the leaf, derive their power from ordinary physical principles. And these considerations also furnish us with another instance of that unity of plan so often met with in the works of Nature; the same law which determines the action of the spongiole determines also the action of the leaf. The same idea is concerned in throwing the sap upward into the stem, and forcing it down again from the leaf. And the rays of the sun, which, by forming that mucilaginous body, gives rise to these concurrent and harmonious actions, equally set in operation the tissues of the leaf which is freely exposed to their influence, and the absorbing mechanism of the spongiole, which is buried, perhaps, many feet deep in the ground.

97. Whatever has been here said respecting the movements of the nutritious juice in exogenous plants, applies also to the case of endogens, the essential mechanism being the same in both instances.

98. It has been clearly established, by the researches of comparative anatomists, that the presence of a circulating machinery is determined by the centralization of the nutritive and respiratory apparatus. In exogenous and endogenous plants, from the circumstance that the liquid and solid material are introduced at distant points, the one through the root, the other through the leaf, channels of communication from one to the other, and, indeed, to every part, are required, and hence the introduction of a circulatory apparatus. In lower tribes of vegetable life, where this separation of function does not exist, the circulatory mechanism is correspondingly absent; the sea-weeds absorb on their whole surface, and nutrition is directly carried forward at the points of reception. In lichens there is the first appearance of a transfusory mechanism, arising from the circumstance that on those parts which are shaded from the light absorption most rapidly takes place; here, probably, however, the channels of movement are the interspaces between the cells, and the cause, simple capillary attraction. In mushrooms there is a more close approximation to the mechanism more fully developed in the higher plants, for in them the rootlets absorb nutrient matter from the soil, from which it passes, by capillary action, to every part of the system.

99. Thus, in the vegetable world, if we commence our examination with the lowest orders of cellular plants, and pass successively through the mosses and ferns to the flowering plants, we see the same physical cause in constant operation. They all obtain their liquid and their solid food through the agency of capillary attraction—the former from the soil, and the latter, in higher tribes, from the air. It is, moreover, in virtue of its action as a capillary mass, that the ground becomes uniformly moist; and hence, through the operation of the same power, fluid matter is first brought to the plant, and then distributed through it. It is in virtue of its capillary diffusion through atmospheric air that carbonic acid gas is brought in contact with the leaves; and when there reduced, it is through the same force that the carbon is distributed to the growing parts. Of the energy of this force there is abundant evidence, not only that it can cause the gradual percolation of juices through small structures, such as the minutest moss, but also lift the sap to the tops of the highest forest trees, and, indeed, far higher were it necessary, and drive it back again to the roots, irrespective of the agency of gravity. Even gaseous substances, as is shown experimentally in the Appendix (Ch. XI.), pass into one another with a force greater than the pressure of a column of water seven hundred feet high; so that, to elevate the sap in a tree, or to drive the blood in an animal, is an insignificant demand on the energy which this force could put forth.

100. In conclusion, we here again remark the influence which the imponderable principles exert in directing all kinds of movements on the surface of our globe. The sap rises in a tree because the sun shines; it is the light of that central orb which produces even these movements in plants. Indirectly, it is true, chemical affinities or electrical agencies are brought into operation, but the prime mover of the machine is the light, which produces a mucilaginous body, which is different in composition in different plants, and which constitutes their proper juices.

101. *The cause of the movement of the sap in flowering plants—both of the rise of the crude sap upward, and of the descent of the elaborated sap downward—is the light of the sun, which effects the decomposition of carbonic acid gas.*

CHAPTER III.

ON THE MECHANICAL CAUSE OF THE CIRCULATION OF THE BLOOD.

CONTENTS: *Ancient Theory.—Description of the Systemic, Pulmonary, and Portal Circulation.—General Law of Movement.—Capillary Relations of Arterial and Venous Blood to the Tissues.—The Systemic Circulation is due to the Deoxydation of Arterial Blood, and its Direction is therefore from the Artery to the Vein.*
Pulmonary Circulation.—Capillary Relations of Arterial and Venous Blood to Atmospheric Oxygen.—Pulmonary Circulation is due to the Oxydation of Venous Blood, and its Direction is therefore from the Venous to the Arterial Side.—Uses and Action of the Heart.
Portal Circulation.—Capillary Relations of Arterial, Portal, and Venous Blood to the Liver.—Three Sources of Force in conducting the Portal Circulation.
Action in Asphyxia.—Case of obstructed Trachea.

102. LET us now proceed to inquire how the physical principles which have guided us, in the preceding chapter, in determining the cause of the flow of sap, apply to the more interesting case of the circulation of the blood in the higher animals.

103. The popular explanation which is given of the circulation of the blood in man, refers to the heart as the prime mover of the mechanism. This central organ of impulse is devoted to a double purpose. It has to throw the blood through the channel of the arteries to every part of the system, and, receiving it back again by the veins, has to throw it to the lungs, in which it must be submitted to the vivifying influence of the air before it can again be restored to the system to be used for the general purposes of the economy. In order to enable it to discharge this task, it is furnished with an appropriate valvular and tubular arrangement, and, at specific periods, contracts and dilates, for the purpose of ejecting or sucking up the circulating liquid. In the opinion of the older physiologists, these periodic motions take place either directly, for the reason that the heart is alive, or, as some of them have supposed, through the mysterious agency of the cerebro-spinal axis.

104. The blood, thus alternately driven from and drawn to this centre of action, is, in this view, a living fluid, possessed of a great many extraordinary properties. A portion of it extracted from the system by any of the ordinary processes of phlebotomy, soon coagulates and dies. In what this change consists, this distinction between the living and dead blood, does not so plainly appear. Moreover, the ancient physiologists imputed a variety of other equally important offices to the heart; they regarded it as the seat of the passions, such as love, and held it accountable for the various deeds in which its possessor was concerned, a moral accountability for good and evil. These philosophical doctrines have, to a certain extent, become interwoven in common speech, and we now often use them without attaching any strict signification to them. Anatomical and me-

chanical considerations might, perhaps, lead us to infer that these ancient views are not altogether correct. There does not seem much consonance between mitral, tricuspid, and semilunar valves, and affections or passions, or the responsibilities for good and evil. A pump may be a very curious and ingenious piece of mechanism, but surely we cannot regard it as a morally accountable agent.

105. For such reasons, therefore, modern physiologists are disposed to cast aside altogether these ancient views, and look upon the heart as an ordinary but beautiful specimen of hydraulic contrivance, in the same way as they look upon the eye as an optical apparatus.

106. In man there are three prominent varieties of circulation: the Systemic, the Pulmonary, and the Portal. It is necessary that we should understand the nature of each.

1st. The Systemic.—Arterial blood, which has been brought from the lungs by the pulmonary veins into the left auricle, is forced by its contraction into the left ventricle, the contraction of which drives it into the aorta. By this it is distributed to all parts of the system, the arrangement of arterial tubes becoming smaller and smaller in diameter until they degenerate into mere capillaries, from which it finds its way into the ascending and descending vena cava, the terminal tubelets of which collect it, and it is returned to the heart by the right auricle during its dilatation. In the systemic circulation, therefore, the blood leaves the left ventricle as arterial blood, is finally distributed by the capillary arteries, undergoes, on its passage into the capillary veins, a chemical change, gives off oxygen, from being bright red it turns dark, and, becoming venous blood, is then brought back to the heart, entering its right auricle.

2d. The Pulmonary.—The venous blood, thus brought to the right auricle, is forced into the right ventricle when the auricle contracts. A similar contraction of the right ventricle now forces it into the pulmonary arteries, by which it is distributed upon the air-cells of the lungs; here a chemical change takes place; from being dark venous blood, it becomes bright red arterial, and, being collected by the pulmonary veins, returns, finally, to the left auricle of the heart. In the pulmonary circulation, therefore, the blood leaves the right ventricle for the lungs, undergoes in them a change, giving off carbonic acid, and then returns to the left auricle.

3d. The Portal.—The portal vein collecting blood from the chylopoietic viscera, distributes it to the liver. This blood there mingles with that which has been derived from the hepatic artery, and which has already been deoxydized in that organ. From this portal blood, bile is secreted and passed into the biliary tubes. The changed blood is now collected by the ramifications of the hepatic veins, along which it is transmitted to the ascending vena cava. The portal circulation is, therefore, apparently not connected with any central organ of impulse. It commences in a capillary system, and terminates in a capillary system. There is no hydraulic mechanism for the purpose of determining a current.

107. Of these different varieties of circulation, let us now select one, and show that the principles we have employed in describing the flow of sap both upward and downward, apply here also. Let us, therefore, confine for the present our considerations to

the systemic circulation, and then proceed to show how the same principles bear on the other varieties also. For, as has already been remarked, it is essential for a true theory of these movements, not only to explain them in an isolated case, but also in every instance; to explain not only the systemic, but also the pulmonary and the portal; to explain not only the varieties of circulatory movement in one given individual, but also in every tribe, no matter whether they be low or high in the scale of creation, or whether they belong to the animal or vegetable world.

108. Let us, therefore, take as our guide the great principle laid down in the preceding chapter (84–88), *That if two liquids communicate with one another in a capillary tube, or in a porous or parenchymatous structure, and have for that tube or structure different chemical affinities, movement will ensue; that liquid which has the most energetic affinity will move with the greatest velocity, and may even drive the other liquid entirely before it.*

109. The arterial blood which moves along the various aortic branches, and is distributed to every part of the system, contains oxygen, which it has derived during its passage through the lungs. Its colour is crimson. As soon as it has reached its destination in the minute capillary vessels, it begins to carry on its proper process of oxydation, attacking in a measured way the various tissues through which it is flowing, burning out their effete carbonaceous matter, perhaps also burning their hydrogen into water. The direct result of this operation is an evolution of heat. But while this chemical change in the tissues is going forward, the arterial blood itself is also suffering a change in giving up its oxygen, which may be looked upon as its active principle, and gaining in exchange the results of the combustion. From being crimson, it turns dark; from being arterial, it changes into venous blood.

110. Let us farther confine our thoughts to what must take place in a single capillary tube, or in one small portion of a porous structure; for whatever reasoning holds in this case, will also hold for any number of capillary tubes, or any mass of parenchymatous structure. On the arterial side of such a tube we have the crimson arterial blood; on the venous side we have the dark venous blood—two different fluids; but what is the relation which obtains between each of these liquids, and the walls of the tube or the substance of the parenchyma in which they are placed? Must it not be that the arterial blood, bearing its oxygen, ready to burn out any carbon or hydrogen in its way, substances of which the tube or structure is composed, possesses an intense affinity for those structures, an affinity which is at last exhibited by its actual destruction of them? The arterial blood, therefore, has an intense affinity for any of the tissues with which it is brought in contact.

111. In the next place, how is it with the venous blood, which occupies the other extremity of the tube? The affinities of the oxygenized—the arterial blood—have been satisfied; it has effected the combustion of the tissues through which it has gone, it has changed into inert venous blood. From being red, it has turned dark. The very change which has come upon it, or which, rather, it has undergone, is sufficient to assure us that, so far as its chemical affinities for the surrounding structures are concerned, those affinities are at an end. The venous blood, therefore, has little affinity for any of those structures with which it is brought in contact.

112. To the mind of a chemist, the relation which exists between arterial and venous blood and the soft solids of the animal body may be very forcibly impressed by these considerations. Regarding the blood itself as a mere vehicle for the introduction of oxygen, and the carrying away of carbonic acid and water, and remembering that the substances of the tissues acted upon are chiefly carbon and hydrogen, our final estimate of the relation of arterial and venous blood respectively to those tissues comes to this—the affinity of arterial blood is expressed by the affinity of oxygen for carbon and hydrogen; the affinity of venous blood by that of carbonic acid for carbon, and of water for hydrogen. Compared together, therefore, the former is the representative of a highly energetic force, which in the latter is diminished down to zero.

113. Now what is the phenomena which our general principle (108) predicts, as arising under these circumstances? Simply this, that the arterial will drive the venous blood before it, and drive it with an inexpressible force.

114. The oxygenizing action of the arterial blood is, therefore, the true cause of the systemic circulation.

115. In the systemic circulation, upon these principles, the flow must be from the artery to the vein.

116. The pulmonary circulation next presents itself. In this, as we have described (106), there is a certain chemical change going on, the consideration of which gives us the cause of the movement. We have seen that the motive force of the systemic circulation arises from the deoxydation of arterial blood (114). How is it with the pulmonary? We have here venous blood presenting itself on the air-cells, no longer presenting itself to carbonaceous or hydrogenous atoms, such as constitute the soft solids, but presenting itself to atmospheric air, or, more truly, to oxygen gas itself, which, being the more absorbable of the constituents of the air, is taken up and held in solution by the moist walls of the air-cells. Under these circumstances, we see plainly that we are considering a case which is precisely the converse of the former; in that the arterial blood had an intense affinity for the carbonaceous substances with which it was brought in contact, and the venous none. In this, the venous blood has a corresponding intense affinity for the oxygen which is dissolved in the tissues with which it is in contact, and the arterial blood has none. Movement again must ensue, but as the conditions of the affinity are reversed, so also is the direction of the motion, for now the venous blood drives the arterial before it, and drives it with an inexpressible force to the heart.

117. The pulmonary circulation is, therefore, due to the oxydation of the venous blood.

118. The direction of the pulmonary circulation ought to be from the venous to the arterial side.

119. Had we, therefore, known nothing of the circulation in the higher order of animals, but been instructed in the chemical relations of the blood to the soft tissues and atmospheric air, we could, upon physical principles, have predicted the existence of that circulation, and shown what its direction in different organs must be.

120. It may strike those who are not familiar with physiological facts, that in these

descriptions a most important omission is made—the omission of the action of the heart, an organ plainly connected by position and mechanism with the phenomena we have under consideration.

121. Physiologists have long seen, in opposition to the popular opinion, that the heart can only exert a very subsidiary action. Plants are wholly destitute of such an organ, yet their juices circulate, and there are multitudes of animals which are in the same predicament. In insects, for example, for reasons for which we can give on these principles a clear explanation, no such central organ of impulse appears. In fishes, the systemic circulation is carried on without a heart, and in cold-blooded animals movement in the capillaries takes place after the heart has been cut out. Even in man, after death, the arterial tubes are found for the most part empty, and it is inconceivable that this should have happened through any possible agency of the heart itself, but meets with a very ready explanation upon the principles we have been giving. In the case of acardiac monsters, also, which are not uncommon, the absence of a heart seems to have exerted little agency on development and growth. Moreover, when we inquire into the condition of the circulation in the earliest periods of existence, we find that, far from the heart being the first to appear as a central point, and its various vessels to branch forth from it, the vessels themselves are the first to appear, and the heart, then, is subsequently developed.

122. What, then, is the true office of the heart? How is it that it comes to form so essential a portion of this mechanism? Had the Creator predetermined that in the construction of the circulatory apparatus of man no organ of the kind should be employed, let us consider the final difficulties which must have arisen. The systemic circulation originates in the deoxydation of the blood, the pulmonary in its oxydation. We can conceive that advantage might have been taken of the excess of mechanical force arising at the points where these chemical changes were going on, for that model was successfully followed in the systemic circulation of fishes; but in the case of animals which do not live in the sea, but breathe atmospheric air, there are serious difficulties in the way. It is very apparent that there is no necessary connexion between the chemical changes taking place in the lungs and those taking place in the system. The rate of oxydation in the lungs depends on a variety of causes, and the rate of deoxydation is also determined by its own proper causes. There is no necessary connexion between them; and if, on either of these points of change, an excess of action took place, the result of it must have been a disturbance of the equilibrium of the whole circulation. At some central point, therefore, the current going to the respiratory machine and that going to the system must be intercepted—and intercepted by an apparatus which could hold both in check, and time the movements of the one to the movements of the other. In animal structures, motions of this kind are universally accomplished by the help of muscular contractions, and hence arose the idea of a heart, which, by periodic muscular contractions, should serve to adjust the flowing currents to one another, and prevent engorgements or deficiencies in any part of the route taking place.

123. From this arrangement, also, another important advantage arose. The arterial and venous tubes in the neighbourhood of the heart attain a considerable diameter.

From this circumstance, they require some arrangement by which they can be filled or emptied. The auricles of the heart relieve all their corresponding veins, to a very great extent, from pressure; the ventricles serve, in their turn, to fill thoroughly the aorta and the pulmonary artery.

124. In the last place, let us turn to the portal circulation, and show how, in it, the same physical principles apply. The blood which flows towards the liver, along the portal vein, has been obtained by that vein from the chylopoietic viscera; it has, therefore, the same relation to the blood furnished from the different and corresponding aortic branches as has the general systemic venous blood. The arterial blood, therefore, drives it before it, in the same way that the general systemic circulation takes place, and, passing along the portal vein, it is now distributed to the liver. In this organ it also receives the blood which has been brought by the hepatic artery, and which has served for the purposes of the liver.

125. The process of biliary secretion now takes place, and compounds of carbon and hydrogen along with soda are separated as bile, and pass along the biliary tubes. In its final effect, therefore, the chemical action of the liver closely resembles the chemical action of the lungs. Compared with the resulting blood which passes along the branches of the hepatic veins, and finds its way into the ascending vena cava, the portal blood differs by containing the elements of bile.

126. Two systems of forces now conspire to drive the portal blood out of the liver into the ascending cava. Let us consider them in succession.

1st. The blood which is coming along the capillary portal veins, and that which is receding by the hepatic veins, compared together, as to their affinities for the structure of the liver, obviously have this relation; the portal blood is acted upon by the liver, and there is separated from it the constituents of the bile; the affinities which have been at work in producing this result have all been satisfied, and the residual blood, over which the liver can exert no action, constitutes that which passes into the hepatic veins. Between the portal blood and the structure of the liver there is an energetic affinity, betrayed by the circumstance that a chemical decomposition takes place, and bile is separated; and that change completed, the residue, which is no longer acted on, forms the venous blood of the hepatic veins. In the same manner, therefore, that in the systemic circulation arterial blood, in its passage along the capillaries, becomes deoxydized in consequence of an affinity between its elements and those of the structures with which it is brought in contact, and drives the inert venous blood before it, so, too, in the portal circulation, in consequence of the chemical affinities and reactions which obtain between the portal blood and the substance of the liver—affinities and reactions which are expressed by the separation of the bile—that blood drives before it the inert blood which is found in the hepatic veins.

2d. But in the liver there is a second agency at work, which, conspiring in its resultant with the former, produces motion in the same direction. As we have said, the blood of the hepatic artery, after serving for the economic purposes of the liver, is thrown into the portal plexus. Hence arises a second force. The pressure of the arterial blood in the hepatic capillaries upon this, is sufficient not only to impel it into

the capillaries of the portal veins, but also to give it a pressure in a direction towards the hepatic veins. For any pressure which arises between the arterial blood of the hepatic and its corresponding venous blood, must give rise to motion towards the hepatic veins; no regurgitation can take place backward through the portal vein upon the blood arriving from the chylopoietic viscera, because along that channel there is a pressure propagated in the opposite direction, arising from the arterial blood of the aortic branches. The pressure, therefore, arising from the relations of the hepatic arterial blood, conspires with that arising from the pressure of the portal blood, and both together join in giving rise to motion towards the ascending cava.

127. So great are the forces which arise under these circumstances, that there is no doubt that the blood of the hepatic artery alone could, of itself, control the circulation of the liver without any re-enforcing aid from the portal blood. So when, from accident or otherwise, the portal vein is shut, or when from malformation it opens directly into the ascending cava, the hepatic artery takes charge of the functions of the liver, and directs its circulatory conditions.

128. These views, therefore, lead us to understand that there are three sources of force engaged, under normal circumstances, in directing the portal circulation. One of these is found in the aortic capillaries, when they are spread on the chylopoietic viscera, the mode of action being precisely analogous to that which obtains in the general systemic circulation. The other two are found in the liver itself; the first is a pressure exerted by the portal blood on that of the hepatic veinlets; the second by the blood of the hepatic artery, which, conspiring with the former, urges the resulting mixture along the hepatic veins into the ascending cava.

129. I might now proceed to show with what clearness these doctrines explain the circulation of the blood in other tribes of life; for example, in the case of the model which is adopted in fishes, the aorta of which has long ago been recognised as bearing a strong resemblance to the portal vein of the mammalia; but, as throughout this chapter these latter have been constantly referred to, I shall continue what is here to be offered farther by using their type of construction for illustration. To any one who reflects on the principles which have been laid down, there will arise no difficulty in explaining the mechanical causes of the circulation in any particular case, more especially if this plain precept is kept constantly in mind, *that, for the physical reasons which have been assigned* (88), *a pressure will always be exerted in every one of these instances by the fluid which is ready to undergo a change upon that which has already undergone it; a pressure which, as there is no force to resist it, will always give rise to motion in a direction from the changing to the changed fluid.*

130. As we have said, it is the character of a true theory to be applicable to all cases, and to render a clear account of every circumstance that may arise. A true theory is like a window of crystal glass, through which we can see all objects in their proper positions, and colours, and relations, no matter whether they are such as are near, or those that are at a distance; no matter whether they are directly before us, or enter only obliquely into the field of view. A fictitious theory is like a Venitian blind, which has to be set in a certain position with respect to the observer, and only shows

him objects for which it has been adjusted, and those in an unsatisfactory manner; but if he moves to one side or to the other, or endeavours to see objects which are not directly in his way, his view is intercepted, or, perhaps, unless he makes a new adjustment, the light is shut out altogether.

131. Let us, therefore, see how these chemical principles will apply when the system under consideration is no longer under normal conditions, but has passed into a state of disturbance or of disease. It is well known to physicians, from the phenomena of asphyxia, that whenever the admission of oxygen into the air-cells of the lungs is prevented, the circulation through them simultaneously stops; but it may be renewed again on the readmission of that gas, provided it is within a short space of time, and recovery may and often does take place under these circumstances. What, now, is the cause of that asphyxiated condition? why does the blood cease to flow? The chemical theory of the circulation of the blood through the lungs, which I have just given, points to the oxydation of that blood as the very cause of its movement (116). It is the pressure of the deoxydized upon the oxydized blood that drives the latter along the pulmonary veins to the heart. But should anything intervene to prevent that oxydation taking place, no pressure can arise, and, therefore, no movement can ensue; the conditions for asphyxia are all present; conditions which, however, are removed so soon as oxydation can be reaccomplished; then movement once more takes place, and a natural state is restored. If any evidence were required to show how little influence the heart possesses in controlling the pulmonary circulation, and how much depends on the oxydation of the blood, it may be derived from the phenomena of asphyxia.

132. As another illustration, we may here bring forward and explain the fact, well known to physiologists, that if the trachea be obstructed so that oxygen is restrained from passing into the lungs, and asphyxia is being induced, blood taken from any of the systemic arteries exhibits a venous aspect. What, now, does the chemical theory say should take place in the general circulation? We are to remember (114) that the very cause of that circulation arises from the pressure of the oxydized arterial blood upon the deoxydized venous blood. Under the conditions supposed, we have interfered with the constitution of the arterial blood, and given it a venous character; the conditions for pressure are, therefore, not accomplished, and no pressure takes place, and no flow from the arterial to the venous capillaries. Under these circumstances, the impulsive action of the left ventricle must be spent in an increased pressure on the walls of all its communicating arteries, and a simultaneous relief of pressure takes effect on the walls of all the veins. As, in a steam-engine, any steady variation of motion is finally impressed upon the governor, which adjusts itself consentaneously; so, in this case, the heart, which acts as a governor, accommodates itself to the changes going on. The left ventricle presently ceases in its violent effort to force the blood, and the pressure on the arterial walls abates. But, if the trachea is now relieved, oxydation of the blood goes on, pressure again takes place in the systemic capillaries, and the proper action is restored.

133. These different facts, which might have been predicted from the theory that the deoxydation of arterial blood is the cause of the systemic circulation, have every

one been experimentally determined by direct experiment by Dr. Reid (*Carpenter's Human Phys.*, p. 417).

134. We might now proceed to inquire what, on these chemical principles, ought to take place when a reverse action ensues, as when between the arterial blood and the tissues through which it flows those conditions are set up which lead to its more rapid deoxydation, and the concomitant evolution of heat; conditions which are found in the various local inflammations. In these, by reason of an increased affinity between the soft tissues and the arterial blood, deoxydation and corresponding combustion more rapidly take place, with an abnormal elevation of temperature, and the flow of blood to the point of disturbance is increased. The old medical aphorism, "*ubi irritatio ibi affluxus*," translated into the precise language of modern chemistry, simply means, *to the point where its deoxydation is taking place, the arterial blood will flow.*

135. By the aid of the principles here laid down, all the various physiological or pathological conditions which are met with in inflammation, asphyxia, gangrene, &c., are presented as so many interesting chemical problems for solution. Such cases, also, as the non-asphyxiation of reptiles, the variable respiration and heat of insects, according as they are in motion or at rest, the results of death from lightning, afford abundant opportunity for the full verification of these doctrines. That the time has now arrived when the exact sciences are to come to the aid of physiology, no one can doubt. For many centuries past, a profitless system has been followed, the same system that formerly obtained in natural philosophy, and the uncertainties and doubts of medical science are the best proofs of its value. It is the ruling principle of this system to satisfy the inquirer for facts by the use of empty words, words which mean nothing and prove nothing. Life and vitality, with other sonorous epithets, figure away in these visionary speculations as though they were realities, and change their forms without reason or rule, as do the images that we see in dreams.

CHAPTER IV.

ON THE PHYSICAL CONSTITUTION OF THE SUNBEAMS AND ON THE PRISMATIC SPECTRUM.

Contents: *Modes of isolating the Coloured Rays.—Newton's Prismatic Spectrum.—Theory of the Colours of Light.—Illuminating Calorific and Chemical Powers of the Spectrum.—Newton's Processes for purifying the Spectrum.—Fixed Lines.—Melloni's Experiments on the Distribution of Heat.—Physical Independence of Heat.—Herschel's Experiments on the Thermic Spectrum.—Chemical Action of the different Regions of the Spectrum on a Daguerreotype Plate.—Chemical Action on other Bodies.*

136. From the foregoing observations (Ch. I.), we see that the primary formation of organized from inorganic matter is brought about by the agency of the sunbeam, either directly falling on the point of change, or received in an indirect way, as the diffused light of the sky or clouds (56).

137. In the sunbeam different principles exist, some of which are visible to the eye and others invisible. The conjoint action of the former communicates to us an impression of what we denominate white light. But in this white light there are rays endowed with the quality of exciting the sensations of colour, such as red, yellow, green, blue. We are required to determine which of these principles is concerned in the physiological change we are considering. It is necessary, therefore, to describe the SOLAR SPECTRUM.

138. A beam of light coming into a room through an ordinary window is perceptible from all parts, for dust or other heterogeneous particles which are always floating in the air scatter the rays in all directions by reflexion, and enable them to operate on the eye. This light constitutes what is designated by optical writers as white light. To the ordinary acceptation of the term white, it does not correspond; it is the unchanged, unaffected light of the sun.

139. A beam of light coming through the painted window of a cathedral falls on the ground, or on objects in its way, and communicates to them the various tints with which the glass has been stained. The brilliant colours which are thus developed by the action of the glass exist originally in the white light, and are made apparent by the absorptive action of the medium through which they have come, in a way which will be presently explained.

140. There are also other modes by which, from white light, brilliant colours can be produced. Thus, transparent media, such as gems, cut into certain shapes and polished, when exposed to the light glisten with a play of colour. It is these brilliant hues which give to the diamond its value as an article of female ornament. It is also in the same way that the angular pieces of glass which are strung upon chandeliers and around gas flames, emit in a brilliantly-lighted room so many fitful changes of colour. And in the same way also Nature exhibits to us that most beautiful of all meteorological phenomena, the rainbow, by refracting, reflecting, and dispersing the white light of the sun, and producing a regular display of colours. These colours, arranged as in the rainbow, may also be seen wherever a shower of drops of water is falling in a proper position as respects the spectator and the sun; they are often, therefore, visible when fountains are playing; often in the sea-spray, when it is cast ashore by a brisk wind; often accompanying the bows of a steamboat which is moving rapidly through the water.

141. There is a third mode by which, from white light, brilliant colours are produced. It is by the interference of rays; in the same manner that two sounds may be so situated with respect to each other as to destroy one another's effect, and produce silence; or, as two waves upon water, when the concavity of the one corresponds with the convexity of the other, destroy one another's effect; so may two rays or waves of light be placed, in respect of each other, that, instead of re-enforcing each other's effect, they may produce darkness. This phenomenon, which in these different cases passes under the general designation of interference, under ordinary circumstances, in the case of light, is expressed by the production of brilliant colours. Such are the beautiful and almost metallic tints that are seen on the wing-cases of certain coleopterous insects, more especially certain beetles in the Southern States, which expose a

glow of colours—greens, yellows, blues, and reds—as they change their position. So, too, the beautiful display which is seen on the breast of the wild dove, and still more beautifully in the feathers of the humming-bird.

142. The cause of these colours, and of the colours of light generally, was first discovered by Sir Isaac Newton, who, in his investigations, followed the second of the methods here mentioned, the production of colour by refraction. Of late years, the attention of philosophers has been much turned to the third method, that of interference; this, as will be presently seen, possesses, in particular cases, very great advantages over that by refraction (140), or the first one, by absorption (139).

143. Newton's methods of investigating the production of colour by refraction may be briefly described, as follows: Let a beam of light coming from the sun (*s, fig.* 111) pass through a circular hole (*a b*) in the shutter of a dark room, and fall upon a piece of glass cut into the form of a triangular prism, *c d e*, placed as in the figure. On the farther side of the prism let there be a sheet of white card-board, M N, or some other such screen, to receive the rays. The light, on its entrance into the room through the hole in the shutter, is white, and were the prism not interposed, it would advance forward on a straight line, and, falling on the screen at *z*, would there give a circular white image of the sun. The interposition of the prism disturbs this white light from its rectilinear course, and, refracting it, bends it into a new path. The disturbance produced by the prism is of two kinds: first, the ray is moved out of the rectilinear position in which it would have gone; and, second, it no longer gives rise to a white circular image, but to an elongated and highly-coloured image, which goes under the name of the solar spectrum. Of the intensity and beauty of these colours it is impossible to give any description by words. An imperfect representation of their position is given in the frontispiece. In the coloured figure thus received on the card-board, Newton detected seven different tints, red, orange, yellow, green, blue, indigo, violet. Of these colours, the red is uniformly nearest to the point (*z*) to which the ray would have gone had the prism not intervened; the violet is most distant. And as the production of the colours depends upon the refracting action of the prism, Newton designated the red as the least refrangible, because it was least removed from its natural course; and the violet as the most refrangible, because it was the most removed.

144. Now if these seven coloured rays be collected together again by any appropriate arrangement, so as all to fall on one common point, that point will be of a brilliant white, as was the point *z*. If two of them, as the yellow and blue, in like manner be directed together, they will give rise to a green; or the red and blue to a purple; or the yellow and red to an orange; showing, therefore, that while different coloured rays thus give rise to compound tints, all the rays in the spectrum converged together produce white light, the same as that which originally came through the aperture *a b*, in the shutter.

145. Newton, therefore, was correct in the explanation which he gave of these phenomena. That the white light of the sun consists of seven differently-coloured rays, some of which are more, and some less, refrangible by glass. That, consequently, when a beam falls on a triangular prism of glass, its constituent rays are not all equally re-

fracted. That of them the red is least, and the violet most refracted, and, therefore, the white image (*z*) which the original beam would have given, became elongated so as to form the solar spectrum in which the constituent colours are seen.

146. Leaving now the optical arrangement by which this phenomenon is produced, let us confine our considerations to the spectrum itself. It is to be remarked that the purity of the constituent coloured spaces becomes greater, as the separation of the rays is more perfect; and, as is shown in *fig.* 111, these rays are diverging from one another, it is obvious that the farther the screen on which they fall is placed from the prism, the greater will be the apparent dispersion of the rays. If the screen is brought close to the prism, the colours are but little developed, because, not being yet separated from one another, their mixture produces white light; but at a distance of twelve or twenty feet, they have diverged sufficiently, and each one appears for itself.

147. Inspection proves that, in this spectrum, not only do the various parts differ in respect of the colours they show, but they also differ in their intrinsic brilliancy. A piece of fine-printed paper, held in succession in the different colours, is legible at very different distances. Held in the yellow, it may easily be read at a considerable distance; but held in the violet, it must be seen close at hand, or the letters cannot be distinguished. The other coloured spaces possess intermediate powers, and, by direct experiments made by Fraunhofer, it appears that the different portions of the Newtonian spectrum have their order of illumination, as is expressed in the following table, in which it will be perceived that the brilliancy of the yellow rays is taken as unity, and the other rays compared therewith. The letters refer to the figure in the frontispiece.

TABLE OF THE ILLUMINATING POWER OF THE DIFFERENT REGIONS OF THE NEWTONIAN SPECTRUM.

Intensity of Light			
In B C	2 1	In E F	32 8
C D	29 9	F G	18.5
D E	100 0	G H	3 5

148. It is not only in illuminating power that these different regions vary; they vary also in their heating power, as is shown by their action on a thermometer. When a prism made of flint glass is used, and the bulbs of a set of small and delicate thermometers are plunged in the coloured spaces, it is perceived that, commencing with the violet, each thermometer rises higher as it approaches the red region; and even beyond the red region, where the eye can detect no trace of light, the maximum of heat occurs, thus showing that the heat which exists in the sunbeam is an intrinsically different agent from the light, because, by the action of a prism, it can be refracted, and is found in a space in which no light exists.

149. It has been known for a long time that the white chloride of silver, and, indeed, all the white salts of silver, when exposed to the sun, turn black (Ap., 443). A piece of paper washed over with any of these white bodies, as the chloride, and held in the spectrum, soon undergoes a change. In the more refrangible region, the rays begin to effect a decomposition, which spreads far beyond the violet extremity; and when the bromide of silver is used, this darkening action is simultaneously begun from end to end of the spectrum, and chemical action extends beyond both of its extremities. In the

same manner, therefore, that from the experiment of Sir W. HERSCHEL, in which a point of maximum heat was observed beneath the red end, the physical independence of light and heat was shown, so from analogous experiments on the chemical changes exhibited by the salts of silver, the existence of a distinct class of rays, invisible to the eye, designated "chemical rays," was established.

150. These general results lead us, therefore, to suppose that there exist in the solar beam a variety of distinct principles, and when that beam is acted upon by a prism of glass, those principles are parted out from each other. Among them some are visible, affecting the eye with the sensation of the various colours of light, red, yellow, blue, &c., and others are invisible, affecting the thermometer, or producing chemical decompositions. The general idea which we gather from these remarks is, that there are three separate principles coexisting in the solar ray, light, heat, and a principle of chemical action; and when this ray is dispersed by a prism, three several spectra arise, of which two are invisible, and one can be seen. Their relative position is such as is given in *fig.* 112, where A B is the luminous, and therefore visible spectrum; C D the invisible, chemical spectrum; E F the invisible spectrum of heat.

151. These are the apparent phenomena which are exhibited when an ordinary solar spectrum is employed. But in that spectrum the several coloured spaces are far from being pure, and many interesting phenomena are therefore imperceptible. NEWTON, who has never been surpassed by any experimenter in minute investigation, studied with great attention the peculiar circumstances which conspire in the production of the ordinary spectrum, with a view of isolating each one of the coloured spaces in a state of purity.

152. First, he shows that, when an image of the sun is formed in a dark chamber upon a screen, as at *z* (*fig.* 111), that image does not terminate in a sharp circular edge, as is the case with the object himself, but is surrounded by a penumbra, the light gradually fading away (*fig.* 113). This can be wholly removed by placing a convex lens so as to receive the ray, and give an image in its focus. And just as the solar image is surrounded by the penumbral ring, so also does the same defect accompany the rays when dispersed by the action of the prism, for the faint light of the penumbra is equally decomposed and dispersed with the bright light of the central image. Such an effect is seen in *fig.* 114.

153. Suppose, now, that by the use of a lens the penumbra is removed, and the direct solar image (*z*) received on the screen is seen to be terminated by a sharp and well-defined edge—the image, of course, will be perfectly circular. If, now, a prism is placed behind the lens, in such a position as to intercept the converging beam of light, as at C (*fig.* 115), where A is the aperture in the shutter, B the convex lens converging the rays that come in through the aperture to a focus, F, where they form a circular image of the sun, the rays are bent on one side by the prism C, and a neat spectrum is formed on the screen at D E, with sharp edges, and devoid of any penumbral continuation.

154. But the image of the sun is circular, and the spectrum itself arises from the separation and successive superposition of coloured circular images, that separation being

due to the different refrangibility of those different coloured rays. Let, therefore, in *fig.* 116, *r o y g b i v* be a series of such circular images, all conjointly composing the spectrum E D, arranged in their proper order, *r* being the red image, and *v* the violet; if we take any one of these images, such as *y*, and consider its constitution, we shall find that, by reason of the overlapping of the adjacent images, there are rays in it which belong to the images on either side, and instead of the circle *y* consisting of yellow light only, it includes orange light derived from the circle *o*, and green light derived from the circle *g*, as is obvious from the overlapping of those circles *o* and *g*, upon the circle under consideration, *y*.

155. Farther, an ordinary spectrum without a penumbra differs in constitution in its various parts. If a line, E D, be drawn through its centre longitudinally, the overlapping of the successive circles along that line takes place to the greatest extent; but if another line, *m n*, be drawn along its edge, on that line the successive circles do not overlap, and, therefore, while along the axis of the spectrum, E D, the intermixture of the rays and of the colours is at a maximum, along the edges of the spectrum there is no overlapping, no intermixture, and each one of the coloured rays is in a state of purity. The light of such a spectrum, therefore, becomes more and more homogeneous as we pass from the axis and go to the outer edge.

156. But suppose the total length of the spectrum remaining the same, those circular images are diminished in diameter, as in *fig.* 117. Here, through that diminution of diameter, overlapping is prevented, and each one of the rays is separated out for itself, there is no superposition, and the colours are equally homogeneous in the axis of the spectrum and on its edges.

157. This diminution in the magnitude of the circular images may be produced in practice in several different ways. The one which Newton recommends, and which is generally adopted, is to use the aperture in the shutter as the radiant source, instead of the sun himself. The diameter of the sun subtends an angle of about half a degree. But over the diameter of the aperture in the shutter we have, of course, a complete control; we can make it half an inch, or the hundredth of an inch, as experiments require. Instead, therefore, of adjusting the screen, E D, *fig.* 115, at such a distance as to receive the image of the sun himself, we remove it farther off, until we have depicted upon it an image of the aperture in the shutter well defined, and with sharp edges On interposing the prism, we find upon the screen, when adjusted to the proper focal point, a spectrum of the aperture, in which the separation of the rays may be carried to any extent by diminishing its diameter.

158. When this diminution is carried too far, the quantity of light admitted becomes very small, and under these circumstances Newton shows that great advantages arise, by employing, instead of a circular hole, a longitudinal slit or oblong aperture; then the separation of colour is equally perfect, and more light being admitted, the different experiments may be made in a satisfactory way.

159. Under certain circumstances, it is desirable to possess a spectrum in which the intermingling of the colours by overlapping takes place in a regular manner. The plan adopted by Newton was to employ a triangular hole. The image of this, when form-

ed by a lens and dispersed by the action of a prism, is as is given in *fig.* 118; where the triangles overlap at the bases, A B, and the intermixture of the different coloured rays is at a maximum, but at the other side of the spectrum the vertex of each triangle being separate from those on either side, the colours are, of course, in a state of purity.

160. These are the chief improvements which were introduced by Newton in the formation of the solar spectrum, and it was in one purified in the manner here described that his most delicate experiments were made. It is extraordinary, that after he had advanced so far as to the use of a lens, and a narrow longitudinal aperture, that the discovery of the fixed lines escaped him. Even if his glass prism was imperfect, these lines are easily seen with prisms made from pieces of looking-glass cemented together and filled with a solution of sugar of lead, and such prisms Newton says that he employed.

161. The dark lines in the solar spectrum were first perceived by Dr. Wollaston, and described by Fraunhofer. They may be seen by forming on a screen of white card-board, *fig.* 2; *Frontispiece,* the spectrum of a narrow fissure or chink. The lens may be a common convex lens, or an achromatic; the prism should be without veins or other flaws; under these circumstances, as soon as the screen is brought into the focal position, so that the spectrum is produced with sharp edges, a number of dark lines will be seen crossing it in certain positions. Of these, a sketch is given in *fig.* 103, and in the frontispiece. These different lines have been designated by Fraunhofer by the letters of the alphabet, as follows: A is in the red region, B in the red near its outer end, C is beyond the middle of the red, D is in the orange, and is a double line, E is in the green, F in the blue, G in the indigo, and H in the violet.

162. Of the fixed lines, it is only the larger which can be seen by projection upon a screen. Viewed through a telescope, a vast number of minute ones become visible. Fraunhofer estimated them at 600, and these are now known to constitute a part only of those which actually exist. In the solar spectrum, they maintain their position among the different coloured spaces, and the same observation applies to the light which comes by reflection from any of the planetary bodies. In artificial lights they are not seen; and in the rays which come from the various fixed stars, although they are present, they are found in different positions and in different groups.

163. It has already been stated that when any salt of silver is exposed to the spectrum (149), a decomposing effect takes place, and the salt becomes black. If the spectrum used be one of a narrow fissure, so that the fixed lines are visible, it will be found that the resulting impression contains them; that where there is a fixed line there is a corresponding want of action upon the decomposed surface; and, farther, that beyond the extreme violet end, and in a region where the eye can perceive no light, the chemical impression reveals remarkable groups of these inactive spaces, M N O P, *fig.* 103. These extra-spectral lines were discovered about the same time by M. Becquerel in France, and by the author of this book. M. Becquerel's description of them was, however, published first. Their use has become essential in all chemical investigations connected with light.

164. Without anticipating here what will have presently to be stated in regard to

the chemical agencies of the solar beam, our attention is first to be directed to its calorific properties. The following facts, which are gathered from the writings of M. Melloni, who has cultivated this department of physical science with such remarkable success, will serve to give an idea of the opinions which have been advocated by that eminent philosopher.

165. The different illuminating power of the different regions of the spectrum is, of course, plainly evident to the eye. Newton supposed that the calorific effect of the different spectrum regions followed the same order, and, therefore, that the yellow space was the hottest. In this view he was followed by Landriani, Sennebier, Rochon, and others. But at a later period, Sir W. Herschel having observed that when different coloured pieces of glass were employed to intercept the solar rays, the amount of heat which traversed them was not in proportion to the amount of light, he made trials on the solar spectrum by putting thermometers in its different colours and marking their elevations. In the course of these experiments he substantiated two remarkable facts: 1st. That below the red region, and in a space where no light could be seen, the thermometer rose. 2d. That the point of maximum temperature was not, as Newton supposed, in the yellow, but totally out of the coloured spaces, and among these dark rays which are less refrangible than the red rays of light.

166. These experiments were repeated by various chemists, and with very contradictory results, some affirming the results of Sir W. Herschel, and others dissenting from them. Seebeck, however, eventually proved that this arose from the nature of the prism used. That when flint glass was employed, the maximum point was in the dark rays; that when crown glass was used, it was at the extremity of the red; for sulphuric acid and alcohol, it was in the orange; for water, it was in the yellow.

167. M. Melloni repeated these experiments with the aid of an improved thermometer, and established their correctness. He took a prism of crown glass, which gave the maximum of temperature at the extremity of the red, and determined the distribution of heat in the coloured spaces, and also in the dark region outside of the red. Then he interposed a layer of water, so that the rays emerging from the prism were transmitted through it, and again measured the temperatures in the obscure and the luminous spaces. They were found altered; some were totally destroyed, others partially, and others had escaped without sensible change. The greatest absorption had taken place among the least refrangible, the action having been less and less as the red ray was approached; it diminished through the red, the orange, and a part of the yellow, and then to the end of the violet it disappeared.

168. The direct effect of this partial absorption is, of course, to move the maximum point, and bring it successively through the red, the orange, and the yellow region. These experiments also show that the heat distributed through the solar spectrum is not a homogeneous agent, for the same medium acts differently on its differently refrangible parts. In the same manner, M. Melloni showed that the calorific rays arising from artificial sources, whether luminous or dark, were heterogeneous, and perfectly like the analogous rays existing in the sunbeam. That while most transparent substances had the quality of absorbing some of these rays, and allowing others to pass, there is one body,

rock-salt, which is equally permeable to them all It therefore constitutes the true glass of radiant heat, and glass and water and other diaphanous bodies act towards heat as coloured media do to light; they possess for it an invisible coloration.

169. When, therefore, we attempt to investigate the distribution of heat in the solar spectrum by the aid of a colourless flint glass prism, we are doing the same thing as if we were attempting to study its coloured spaces by employing a prism of blue or green glass. Such an instrument would, of course, wholly disturb the proper constitution of the spectrum, absorbing some rays and letting others pass; and as it acts towards light so does flint glass act towards heat, because it possesses an invisible coloration for that principle. But rock-salt, which is transparent equally to all these rays, will not produce such an effect, and hence, in all investigations connected with radiant heat, prisms of that mineral ought to be employed. When this is done, it is found that the maximum of temperature in this normal spectrum exists in the dark space, not in contact with the red extremity, as Sir W. Herschel supposed, but wholly detached from the colours, at a mean distance equal to that which exists in the contrary direction between the red and the yellow. If the rays which form this normal spectrum are made to pass through a plate of flint glass of sufficient thickness, the maximum approaches towards the red region; if one of ordinary glass, it passes into the red; if water or alcohol be used, it enters the beginning of the yellow. But, by reason of the limpidity of these different media, the colours undergo no sensible alteration, and the maximum remains always invariably fixed at the beginning of the yellow.

170. Thus, the inferior bands of the spectrum may preserve the same ratio of luminous intensities, and lose the relations which exist in their temperatures. The calorific elements do not follow the lot of their corresponding luminous elements. Therefore, light and heat are two different agents, or two modifications essentially distinct of one common agent.—(Melloni, *Comptes Rendus*, Jan., 1844, p. 39, &c.)

171. To these beautiful results of Melloni, Sir J. Herschel (*Phil. Trans.*, 1840, p. 52) has added other very remarkable ones, obtained by a different process. If we take a slip of thin writing paper, and, having blackened one side of it by exposure to the smoke of a candle, adjust it so as to receive the solar spectrum, and with a flat brush equal in breadth to the paper dipped in rectified spirits of wine, wash over the white surface until the paper is completely saturated, and looks of a uniform black colour, "a whitish spot begins to appear below the extreme red end of the luminous spectrum, which increases rapidly in breadth until it equals the breadth of the luminous spectrum, and even somewhat surpasses it, and in length till it forms a long appendage exterior to the spectrum, and extends, moreover, within it, till it reaches up to, and beyond the centre of the yellow ray. In this state, and just as the general drying of the paper begins by whitening the whole surface to confuse the appearances, a second sudden and copious wash of alcohol from above downward must be applied, without disturbing the spectrum, or in any way shaking the apparatus. The superfluous alcohol will have hardly run off, when the phenomena of the thermic spectrum begin to appear in all their characters; at first faintly, and, as it were, sketched in by a dimness and dulness of the otherwise shining and reflective surface of the wetted paper; but this is speedily

exchanged for a perfect whiteness, marking by a clear and sharp outline the lateral extent of the calorific rays, and by due gradations of intensity in a longitudinal direction their law or scale of distribution, both within and without the luminous spectrum."

172. "The most singular and striking phenomenon exhibited in the thermic spectrum thus visibly impressed is its want of continuity." It consists of several distinct patches, or round solar spots, as is seen in *fig.* 119.

173. These are the leading facts in relation to the distribution of heat in the Newtonian spectrum. Let us, in the next place, proceed to examine the laws of the distribution and action of the chemical rays, commencing first with the decomposition of one of the salts of silver, as, for example, the iodide of silver, which forms the basis of the Daguerreotype.

174. The action of the Newtonian spectrum on the Daguerreotype plate is very remarkable. Imperfect attempts were made, soon after the preparation was published, to determine the changes which happen to this very sensitive substance. For the most part, these, however, were made without any kind of precaution as respects the purity of the dispersed colours—precautions which Newton, a century ago, so clearly pointed out. Instead, therefore, of spectra of great purity, in which the length is many times greater than the breadth, drawings were given in which the breadth of the solar image is equal to its length. Whoever remembers that, in the experiments conducted by Newton with a view of determining the phenomena of homogeneous light, and in which the lengths of his spectra were sometimes seventy times greater than their breadth, will easily understand how small must be the value of such imperfect proofs, which, indeed, can scarcely be called impressions of the solar spectrum.

175. The impressions obtained in July, 1842, in Virginia (Ap., 645–647), and which may be taken as almost perfect specimens of the action of this variety of spectrum, were made by strictly following the precautions of Newton (152, 153). Several of them were obtained, representing the action under various changes in the dimensions and figure of the aperture admitting the light; as, for example, when it was a circle, a triangle, a fissure. Of the former of these, a very elaborate account has been given in the Philosophical Magazine, by Sir J. Herschel (*Phil. Mag.*, Feb., 1843), who has deduced from it what is probably the true theory of the action of light on the Daguerreotype plate. In these perfect spectra there are certain peculiarities which cannot at all times be produced; they require an exceedingly brilliant sun, and a clear sky. In tropical latitudes there is no difficulty, during the heat of summer, in obtaining them; but in New-York, during a great many trials which I have made in the course of two years, I have but once been able to reproduce them.

176. The solar spectrum obtained upon iodide of silver may be roughly divided into two great and almost equal regions: the upper region, corresponding to the more refrangible rays, embraced between the blue and the extreme violet, exhibits, by a deep dark colour, or series of colours, that a decomposition of the iodide has taken place; probably in those portions in which this decomposition has been complete, the iodide has lost one half of its iodine; the lower region, which is by far the most interesting portion of the two, and by far the most difficult to study, corresponds to the lesser re-

frangible rays, which are included between the extreme red and the blue. There is no difficulty, at any time, in securing the representation of the upper, but it is in this lower part that the diversity of result is so great, and about which there is so much difference of opinion among different authors.

177. I regard these two classes of rays—the more and the less refrangible—as exhibiting, upon the iodide of silver, antagonizing and contrary actions, the former exerting a decomposing agency, the latter a protecting agency. The circumstances under which the lower region makes its appearance are only found when the sky has that degree of brightness that the sensitive surface is slightly stained by it; the spectrum rays then exert their protecting agency on the places on which they fall, and maintain the iodide in an undecomposed state. That this is the true statement seems clear to me, from the circumstance that the phenomenon is independent of time. It is immaterial whether we expose the sensitive surface for five minutes or for an hour, no change whatever takes place in this lower region; the iodide remains equally undecomposed. Now this must arise from the circumstance that the decomposing effect of the skylight is exactly balanced by the protecting agency of the lower rays—so exactly balanced that it is immaterial whether the exposure be for one minute or for an hour, the resulting action is the same.

178. This view of the relation between the more and less refrangible rays, in the action on iodide of silver, is strengthened by what is known to take place when phosphorescent surfaces are exposed to the sun. In that case, as all writers agree, the lesser refrangible rays can not only exert a protecting agency, but even extinguish the phosphorescent glow occasioned by those which are more refrangible.

179. Exposed to the prismatic spectrum, different compound substances give rise to very different results. In some cases the change is limited to one, in others to other regions of the spectrum. Thus the iodide of silver seems to be decomposed by the more refrangible rays, the chloride exhibits a greater range of the same species of action, the bromide is blackened by every ray from end to end of the spectrum. Of these different results a very extensive series has been made known. They have occupied much of the attention of Sir J. Herschel, who has published the results of his investigations in the Transactions of the Royal Society.

180. *Fig.* 120 shows such impressions on different substances, *a* being the effect on the iodide, and *b* on the bromide of silver. In these it is to be remarked, that not only does the length of the figure vary very greatly, but also the position of the maximum point.

CHAPTER V.

ON THE INTERFERENCE SPECTRUM.

CONTENTS: *Defects of the Prismatic Spectrum.—Mode of forming the Interference Spectrum.—Its Peculiarities.—The Distribution of the Colours, and Law of their Intensities.—Reflected Interference Spectrum.—Its Fixed Lines.—Wave-lengths of the Seven Great Rays.*

Melloni's Researches on the Distribution of Heat in Perfect Prismatic Spectra.—Apparent Identity of Light and Heat.

Distribution of Chemical Force in the Interference Spectrum.—Comparison of the Fixed Lines in the Prismatic and Interference Spectrum.—Mode of Defining Chemical Effects by Wave-lengths or by Times of Vibration.—Impression on Bromide of Silver.—On Chloride of Silver.—Total Change in the Distribution of Heat in the Interference Spectrum.

181. THE prismatic spectrum is obtained in a state of the greatest purity when the decomposed beam of light comes through a narrow fissure, and the instrumental arrangement given in (153) is employed. All the fixed lines are then thrown upon the screen, and the separation of the different homogeneous rays is accomplished, perhaps, as perfectly as is possible.

182. But in this spectrum, as employed for investigating the chemical action of the solar beam, very serious difficulties may be traced. If we inspect a photographic impression, or if we consider the intensity of the luminous rays in its different parts, we perceive that, as the violet end is approached, the different changes take place in a less abrupt manner; the light fades gradually and imperceptibly away, the photograph does not end sharply, but, spreading itself out to a great distance, disappears so gradually that we can scarcely say where its termination is, and the fixed lines increase in number and breadth compared with what we perceive at the red extremity.

183. These different results obviously arise from an inherent defect in the prismatic spectrum; a defect which originates in the very cause which gives rise to the spectrum itself—unequal refrangibility. Of two sets of rays compared together, one set taken in the red, and the other in the violet region, it is obvious that, in the same spectrum, from the very circumstance of their greater refrangibility, those in the violet will be relatively more separated from each other than those in the red. That is to say, if we have two rays, *a* and *b*, in the red, whose indices of refraction are almost identical, and which, projected in the spectrum, would stand close to each other, side by side, and another pair, *c* and *d*, in the violet, whose indices of refraction bear precisely the same relation to one another, these last, by the act of refraction, will be more separated, because they are relatively more refrangible than the former. The result of this increased separation in the more refrangible regions is to give an apparent dilution to them, while

the lesser refrangible regions are concentrated. The relative position of the colours must also vary; the fixed lines must be placed at distances greater than the true distance, as the violet end is approached.

184. In all investigations on the chemical action of the spectrum, the greatest importance is to be attached to these considerations, for our estimates of chemical results depend on the amount of action taking place in a given time. When, therefore, we obtain a prismatic impression on any sensitive surface, it is very far from representing the true character of the phenomena. The action which ought to be concentrated in a lesser space at the more refrangible region is spread over a greater, and with that augmentation an apparent diminution of the amount of action is perceived. This, of course, should make the maximum point vary, spread out unduly the violet end, and dilute the true effect. The different regions of the prismatic spectrum cannot be fairly compared with one another.

185. My attention having been directed to these peculiarities, I obtained from the mint at Philadelphia, in May, 1843, a piece of ruled glass, with a view of examining the different questions for which solutions had been apparently obtained by the defective use of the prismatic spectrum, substituting for it the interference spectrum of Fraunhofer. I am particular in mentioning this date, because I perceive that, at the very time that I was making these trials in New-York on the chemical agencies of the spectrum, Professor Masotti was doing the same thing for the luminous rays in Italy, as is stated by M. Melloni, in the *Comptes Rendus*, Jan., 1844, p. 44; and it is possible that, before the time that this volume reaches the Continent of Europe, other skilful experimenters may have followed in the course of Professor Masotti, and, perceiving the great advantages which arise, may have applied it for the chemical rays.

186. As the interference spectrum is less known than the prismatic, I will here give a brief account of the mode by which it is to be formed for experimental purposes, and of its peculiarities.

187. The principal apparatus is a grating composed of a number of intervals, alternately transparent and opaque. A piece of glass, with lines ruled on it with the point of a diamond, forms the best grating; these lines should be parallel to one another, equidistant, and so close that several hundreds, or even thousands of them, may be comprised in an inch. To so great a degree of perfection did Fraunhofer carry this kind of work, that he obtained gratings containing 30,000 lines in the inch. A beam of light, directed horizontally into a dark room by means of a heliostat, is to be transmitted through a fissure, as in the arrangement for showing the fixed lines in the prismatic spectrum. At a distance of several feet, the ray is received on the ruled glass, behind which, a few inches intervening, a piece of ground glass is placed. The lines which are upon the glass are to be adjusted so that they are parallel to the sides of the fissure through which the light enters. In Fraunhofer's arrangement, instead of a ground glass, a small achromatic telescope was placed, having a mechanism for measuring angular deviations, and rotating on an axis coincident with the centre of the grating; the following facts, which may less exactly be verified with a ground glass, were then determined. Upon the axis of the ray coming in through the fissure, a white

image of that fissure was seen; its sides were perfectly sharp, and of the same appearance as though the grating had not intervened; on the right and on the left of this image, two equal spaces, completely dark, and, beyond each of these spaces, a series of solar spectra, each having its violet extremity pointing inward towards the central image, and its red extremity outward. Of these spectra, the first on each side is perfectly insulated, but the violet of the third projects upon the red of the second, and in the same way each successive spectrum is overlapped by those coming after it. These spectra are situated symmetrically on each side of the central image, and with a telescope, or when other proper means are used, the fixed lines are plainly visible in them.

188. In *fig.* 121 these phenomena are represented: A is the central white image; B C D. . . . the successive spectra on the right hand; E F G, the symmetrical ones on the left.

189. If, now, we measure the distance of any one of the fixed lines, for example H, from the middle of the central image in the successive spectra on one side, we shall find that the distances which separate that centre from this ray in the consecutive spectra on one side—H′ in the first, H″ in the second, H‴ in the third spectrum—are in an arithmetical progression; A H″ is double, A H‴ is triple of A H′. The angular separation, measured by the instrument, between a given ray and the middle of the central image, is termed the deviation.

190. "Fraunhofer has measured thus the deviations of the principal lines, making use of different gratings; that is to say, of gratings the lines of which were more or less close, and in which the opaque groove was more or less large compared with the transparent interval. He has proved, in this manner, that the deviation of the same line, or of the same colour, does not depend on the ratio of the width of the groove to the width of the transparent interval, but on the sum of those two magnitudes; that the absolute value of the deviation is in the inverse ratio of this sum; that is to say, that in multiplying the measured deviation by the known sum of the width of a groove and a transparent interval, we obtain a number which is constant for the same ray, whatever may be the grating which we use. Fraunhofer has calculated upon exact and numerous experiments the values of this constant for the seven principal lines of the solar spectrum, and finds that these values are precisely equal to the lengths of undulations of colours corresponding to those rays, such as Fresnel had obtained by other processes." —(Lame′.)

191. Of these phenomena the undulatory theory gives a rigorous account. It farther shows, that the deviation of one of the colours in the first spectrum, multiplied by the sum of the breadth of one of the grooves of the grating, and of its corresponding transparent part, is equal to the length of an undulation of that particular colour. This product, therefore, is constant for all gratings, and gives an easy means of determining the length of an undulation.

192. In the same spectrum, also, the deviations of any two colours are to one another as the lengths of their respective undulations. For this reason, therefore, the violet ray is situated nearest to, and the red farthest from the optical axis.

193. From a note attached to M. Melloni's memoir (*Comptes Rendus*, Jan., 1844,

p. 43), it appears that Professor MASOTTI has determined that in these beautiful spectra the most luminous point is in the centre of the yellow ray, which is itself placed at an equal distance from each extremity of the spectrum. From this central point the luminous intensity declines regularly and equally to each end, so that the red limit and the violet limit have both the same luminous intensity, and are the least luminous parts of the spectrum. M. MASOTTI has also shown that the colours of these two limits are formed by ethereal waves, the lengths of which are respectively to each other in the remarkable proportion of 2 : 1. On the frontispiece the interference and prismatic spectra are compared side by side.

194. Before directing farther attention to the peculiarities of the interference spectrum, I shall proceed to explain the method of obtaining it which I have found most suitable for chemical purposes. Through a narrow fissure, A, *fig.* 4, frontispiece, I direct a beam of light horizontally by a heliostat, and at a distance of twelve feet I receive it upon the surface of a piece of ruled glass, the lines of which are parallel to each other, and rigorously parallel to the heliostat fissure, A. More than a year ago I found that there are practically great advantages in using a reflecting grating over one in which the light is directly transmitted. The glass, B C, is therefore to be silvered with a piece of tin foil, so as to act like a mirror, and it will be found that the tin amalgam upon it copies perfectly all the ruled lines. In this mode of operating there is, therefore, no difficulty in placing B C so that the ray coming from A falls perpendicularly on it, for all that is required to ensure the proper adjustment is to move B C until it is in such a position that the ray, after reflexion by it, returns back to the heliostat fissure, A. At a distance from B C of six inches, I place an achromatic object glass, D, in such a position that it shall receive perpendicularly the reflected rays of the spectrum of the first order; in my arrangement this happens to be the left-hand spectrum, as the observer looks towards the heliostat fissure, A. The achromatic lens is thus brought as near to the grating as possible, without its edge intercepting the ray coming from A. In the focus of this lens, at E F, a ground glass is placed; it is adjusted in a box, E G, and so arranged that the ground glass may be removed, and any photogenic surface put in its place. This portion of my apparatus is, however, nothing more than the sliding part of a common Daguerreotype camera, which contains the necessary ground glass and cases for photogenic preparations.

195. The apparatus being properly adjusted, so that the ground glass receives the rays perpendicularly, the spectrum will be seen depicted upon it in beautiful perfection, exhibiting all its striking peculiarities and its fixed lines quite visible. Every precaution should be used to shut out all extraneous light.

196. The descriptions given of the interference spectrum by optical writers refer to the transmitted spectrum; but these which are obtained by reflexion are, as FRAUNHOFER showed, like those in all respects. For several reasons, it would be preferable to use parallel lines ruled on a piece of polished steel or speculum metal. Silvered glass, however, has the advantage of not changing readily under the influence of the various corrosive vapours used in these researches. The grating which I employ is five eighths of an inch long, and one third of an inch broad.

197. The interference spectrum is given in *fig.* 133 and in the frontispiece, with its fixed lines and coloured spaces, as seen on the ground glass. By the side of it is placed the prismatic spectrum, the spectrum of NEWTON. Compared together, we see that in the latter the yellow region is among the less refrangible rays, in the former it occupies the central position of the spectrum. The distribution of the fixed lines is also different, though their position, as respects the coloured spaces, is the same for both spectra. Of course the light is distributed differently in respect to its intensity in the two spectra, as has been said: for the one it is a maximum in the centre, and fades away equally to each extremity; but for the other, the illuminating power is as given in the table (147) derived from FRAUNHOFER.

198. The following table, derived from Sir J. HERSCHEL'S Treatise on Light, gives the lengths of the waves of light corresponding to each one of the principal lines of the spectrum. The Paris inch is supposed to be divided into one hundred millions of equal parts, one of which constitutes the unit of the scale.

Length of wave	corresponding	to the ray	B,	2541
"	"	"	C,	2422
"	"	"	D,	2175
"	"	"	E,	1945
"	"	"	F,	1794
"	"	"	G,	1587
"	"	"	H,	1464

199. In the preceding chapter, we have given a brief account of the experiments of M. MELLONI on the distribution of heat in the spectrum, and shown (170) how he deduced from those results the doctrine of the physical independence of the two imponderable principles. More recently, however, M. MELLONI has repeated the same experiments (*Comptes Rendus*, Jan., 1844), and draws from them a result diametrically opposed to the former.

200. That a philosopher should, with the progress of knowledge, change his opinions on fundamental points of science, is very far from being a fault. For more than ten years M. MELLONI has studied, with unexampled success, the laws and phenomena of radiant heat. The leading doctrine which he has deduced from those researches is that to which we have just referred, *the entire physical independence of light and heat.* From recent repetitions of his former experiments, he has now arrived at a conclusion apparently opposite, "that light is only a series of calorific indications sensible to the organ of sight, or, *vice versâ*, that the radiations of dark heat are true *invisible radiations* of light." The arguments which have been employed (169) in discussing the experiments of SEEBECK and others, on the shifting of the maximum point when different diaphanous media are used from the red into the orange, or even into the yellow, all hang upon the admission that the spectrum under consideration is sufficiently pure, and that there is no superposition or overlapping of its parts. For, were this the case, even where colourless and diaphanous media are used, such as crown-glass, water, sulphuric acid, and alcohol, though these, by reason of their transparency, do not exert any absorbent action on the rays of light, they might, by reason of their invisible col-

oration, absorb certain of those rays which are beyond the extreme red; and if these obscure rays, by superposition or overlapping, extended from their proper places into the coloured regions above, the absorptive action of the diaphanous media would then be exercised, and apparent movements of the point of maximum take place, such as have been actually observed—the point shifting successively from the obscure spaces into the red, from the red to the orange, from the orange into the yellow.

201. Now, in those spectra which were formerly employed, both by Sir W. Herschel and M. Melloni, this very difficulty must have taken place, and the results obtained were therefore not simple, but compound. It is obvious, if we would avoid these interferences, that we must employ a spectrum capable of showing the fixed lines, the parts of which are properly separated from one another.

202. On making use of such a spectrum, M. Melloni found the maximum temperature uniformly at the extremity of the red ray, it being immaterial what diaphanous substance had been used as a prism or as an absorbent medium. When glasses of a brownish colour were interposed, a colour which results from their exerting an absorbent action on all the colours of the purified spectrum, it appeared that so intimately were "these colours allied to their temperatures, that during the transmission they lost as much heat as light, so that the ratio of these two agents remained always unalterable." From these things it is deduced, "That the luminous radiations, disengaged from every other heterogeneous radiation, have a heat of their own, which follows exactly the same vicissitudes, so that the different phases of a given ray of simple light may be measured indifferently by its luminous or calorific relations."

203. Of all the departments of physical science, there is none about which so many experimental difficulties gather as that which is connected with investigations on the solar spectrum, which is the point of reunion of the most energetic and powerful imponderable agents, visible and invisible; and probably we shall not obtain true views of its constitution and properties until our doctrines have changed over and over again, and years have elapsed. In the views just cited, M. Melloni returns to the opinions held at the beginning of this century; those views which led to the invention of Leslie's photometer.

204. That some splendid generalization will hereafter unite all these imponderable principles, we have repeatedly said; but there are very many facts now known which none of the views hitherto brought forward can embrace. Under these circumstances, it would seem that the proper course to pursue is to regard each one of these agents as physically distinct. Notwithstanding the clearness with which M. Melloni has put forth his recent doctrines, those who have read with attention the beautiful memoirs which he has formerly written, will pause before they assent unconditionally. It is not by a difficult and delicate experiment, such as that now under consideration, nor even by a series of them, that that assent will be readily obtained. There are former experiments to be explained away, former measures to be accounted for, before this desirable simplicity can be effected. It is very true that, admitting the doctrine of the identity of light and heat. or their common dependance on one higher agent, the experiment we have just described meets with a ready explanation; but we become involved in difficulties

when we attempt similar explanations for other facts. Thus, a thin plate of transparent alum and a thick mass of smoky rock crystal, the former completely diaphanous, the latter so opaque that even when placed in the fullest light it was not possible to read large printed characters, being compared together, the latter was found to be more transparent to heat than the former in the proportion of 19 to 6; and, indeed, if these agents be identical, and the rays of light have a proper heat of their own, so that their amount may be measured by a thermometer, what becomes of such experiments as the following, brought forward, on the other side of the question, by M. Melloni, formerly, who passed a ray of light through a stratum of water, and then through a glass tinged green by the oxide of copper? "The *pure light* emerging from this system contains much yellow, and possesses, at the same time, a tinge of bluish green. *It exhibits no calorific action capable of being rendered perceptible by the most delicate thermoscopes, even when it is so concentrated by lenses as to rival the direct rays of the sun in brilliancy.*"—(*Taylor's Scientific Memoirs*, vol. i., p. 392.

205. Until, therefore, an explanation can be given of this experiment, or its authenticity disproved, the recognised rules of chemistry require that we should speak of light and heat as distinct agents. Nor are these the only imponderable principles which are involved; there are others known to exist, the position of which is determined by these discussions: the chemical rays, for example, which M. Becquerel regards as nothing more than invisible light, adopting for them the same theory which M. Melloni has adopted for the rays of heat. There are also rays which can excite the glow from phosphorescent bodies. From the time of Franklin and Canton it has been known, that if calcined oyster shells were exposed to the rays of an electric spark, as, for example, to the discharge of a Leyden vial, the shells would commence to shine; but, as may easily be proved, it has more recently been found, that if the rays thus exciting the phosphorescent quality be transmitted through a piece of colourless flint glass, they can no longer produce the result. It might be said that this arises from the fact that colourless glass has an invisible coloration, and that it absorbs the rays which are far beyond the violet, but direct experiment proves that flint glass possesses no such quality.

206. Inasmuch, therefore, as a piece of flint glass can cut off those dark rays which excite phosphorescence in the sulphuret of lime, and combinations such as that which has been referred to (204) are known, which can separate light from heat, we are justified in regarding these as distinct imponderable principles. They may, it is true, be remotely identical, but they are not identical in the way here set forth. They may all consist of similar movements or undulations of one primordial ether, but there are points in which the mechanism of those undulations vary—circumstances which impress upon them physical differences. As, in the phenomena of sound, we may have instruments which give rise to undulations of the same length and the same intensity —instruments which are executing the same strain of music at the same time—yet we know well enough that each one impresses its own proper modifications, which the ear in an instant detects. At a distance, we recognise the flute, the violin, the piano, the bugle, from one another.

207. The imperfections of the prismatic spectrum, which have been just described

as exciting so powerful an influence in the distribution of light and heat, are equally perceptible in the case of chemical effects, a result which becomes apparent at once when we operate with the interference spectrum. When, on any sensitive silver surface, this spectrum is received, instead of a stain exceeding in length the coloured spaces, the change is limited to a narrow region, occupied by the more refrangible rays.

208. In *fig.* 133, the visible interference spectrum is given with its fixed lines, as it appears on the ground glass of the camera. The figure in the frontispiece is drawn from the measures of Fraunhofer. If the two be compared together, it will be seen that there are differences in the relative distances of the lines. These differences arise from the mode of conducting the experiment in the two cases. Fraunhofer's spectrum was carefully determined, the angular deviations being measured on a graduated arc. *Fig.* 133 is depicted from the appearance on the flat ground glass of the camera.

209. Before giving a description of the chemical effects of this spectrum, it is necessary to explain the method of subdividing the spectrum which is here resorted to. In the earlier discussions on the chemical effects of light, the different regions of the spectrum were marked out by the designations of the different coloured rays, and effects were described as taking place in the red, or yellow, or violet ray. An improved plan was proposed by Sir J. Herschel, and followed by him in his various writings: it consists essentially in dividing the space which exists between the red and yellow ray, as insulated by cobalt blue glass, into 13·30 parts, taking the centre of the yellow ray as the zero point, and continuing the divisions equally into the more and less refrangible regions.

210. Over these different methods the use of the fixed lines possesses very great advantages, inasmuch as we do not make reference to ideal, but to actually visible points existing in the spectrum. Since the discovery of these lines, therefore, both M. Becquerel and myself have used them to mark out spectrum regions. The only difficulty which is in the way is, that they do not give subdivisions minute enough for many purposes. But this difficulty can be wholly removed, and other very great advantages gained, by using them in the manner which I shall now proced to explain.

211. It has been stated that the deviations (189) of the different fixed lines, B, C, D, in the interference spectrum, are proportional to the lengths of the undulations which they respectively represent. By designating the different points of the spectrum by their wave-lengths, the subdivision may be carried to any degree of minuteness; the measures of one author will compare with those of another, and the different phenomena of chemical changes occurring through the agency of light become allied at once with a multitude of other optical results; as, for example, when we are told that the decomposition of bromide of silver occurs at a maximum under the influence of a ray which is 0·00001538 of a Paris inch in length, we recognise in an instant that this ray falls between the fixed lines G and H, the length of G being 0·00001587, and of H 0·00001464; we see, moreover, that the point spoken of is nearer to G than to H, and, if it were necessary, by a very simple arithmetical process, we could determine the number of vibrations executed by that ray in thus bringing about the maximum decomposition in billionths of a second, using the rate of the propagation of light at 192,000 miles.

212. The fixed lines, used in this way, enable us at once to divide the interference spectrum into any number of parts, and to indicate effects either in space or in time. For chemical purposes, in which mathematical accuracy is scarcely attainable, all that we have to do, in order to determine the lengths of waves producing given effects, or the times of vibration, is to determine, upon the interference spectrum, the point at which the change in question has taken place, and, using the numbers which FRAUNHOFER measured for the different fixed lines, find what is its relation to them. Thus, for example, suppose it has been found, by experiment, that a certain substance, exposed to the interference spectrum, exhibited a maximum point of decomposition exactly midway between the fixed lines F and G, which are both impressed on it, it is required to know what is the length of the wave which brought about that decomposition in parts of a Paris inch. The numbers given by FRAUNHOFER are,

For G	0·00001587,
" F	0.00001794;

and the point in question, being midway between the two, has for its wave-length 0·00001690.

213. In the frontispiece, I have annexed FRAUNHOFER's numbers to each ray, omitting, for the sake of brevity, the ciphers.

214. In *fig.* 134 is represented the effect of the interference spectrum upon a silver plate, rendered sensitive by exposure to iodine vapour, and then to bromine. The time of its exposure in the camera was half an hour. The point of maximum falls, as may be determined upon the foregoing principles, nearly at the point 0·00001538. The dotted lines, x and y, indicate the beginning and end of the stain. The maximum point does not fall equally between the two, but is nearer to the more refrangible extremity. When this spectrum is compared with the corresponding prismatic one, in *fig.* 103, we see how great is the difference; the effect, which is there carried far beyond the extra-violet regions, is here compressed down into a narrow space.

215. *Fig.* 135 represents a very beautiful result, obtained on a silver plate exposed first to the vapour of iodine, and then for a short time to the vapour of chloride of iodine. In this case the point of maximum falls nearer the line G. The time of exposure was ten minutes. The fixed lines, which were discovered by M. BECQUEREL and myself beyond the visible spectrum, are here crowded down into so small a space that the individual groups are all found together, so that they would scarcely be recognised.

216. *Fig.* 136 gives a very perfect result which I obtained on a plate exposed first to the vapour of iodine, then to bromine, and then to chloride of iodine; the point of maximum falls at 0·00001538, as in *fig.* 134; the time of exposure was one hour. The decomposition extended on one side beyond E, in the green space, to the point 0·00002007; and beyond the violet space to the point 0·00001257. As in *fig.* 134, we here again see the point of maximum is not in the middle of the image, but is towards its more refrangible extremity.

217. As respects this spectrum (*fig.* 136), although it extends to the yellow space, the line E is not represented in it. I may remark of these spectra, as was formerly remarked (AP., 746–747) in the case of the prismatic, that the lines D and E do not ap-

pear. M. BECQUEREL, as appears from the Scientific Memoirs, vol. iii., pl. ix., *fig.* 2, has been more fortunate. Although, in the course of a great many experiments, I have often obtained very beautiful results, in which the lines in the indigo and violet spaces were given in great numbers, and many of them of that degree of minuteness as to require a lens to show them distinctly, it has never yet happened to me to see the fixed lines D and E reproduced on a sensitive surface of any kind.

218. It may be remarked, in conclusion, that just as we see the colorific rays symmetrically arranged in these interference spectra, and the chemical results expressed by the decomposition of chloride and bromide of silver crowded into a narrow space, compared with what takes place in the prismatic spectrum, so must the same thing hold for the rays of heat, the apparent distribution of which must be totally altered.

CHAPTER VI.

EXPERIMENTS PROVING THAT IT IS IN THE YELLOW REGION OF THE SPECTRUM THAT THE REDUCTION OF CARBONIC ACID BY THE LEAVES OF PLANTS TAKES PLACE.

CONTENTS: *Several Imponderable Principles in the Sunbeam.—Sennebier's Experiments to determine to which of these the Effect is due.—Experiments of Morren and Daubeny.—Defects of the Mode of operating with Absorbent Media and Glasses. Decomposition of Carbonic Acid in the Prismatic Spectrum.—Process of conducting the Experiment.—It is in the Yellow Region that the Decomposition takes place.—No Gas is evolved in the Violet.*

219. THE elementary views of the constitution and nature of the spectrum which we have given enable us now to return to the physiological problem under discussion. From Chapter II. we see that the process of digestion of plants, so far as we have yet examined it, may be separated into three different chemical actions. 1st. The absorption of carbonic acid and water. 2d. The evolution of a mixture of oxygen and nitrogen. 3d. The retention by the plant of carbon, hydrogen, oxygen, and nitrogen.

220. There has, therefore, been a decomposition of carbonic acid, of water, and probably of some nitrogenized compound.

221. But these extraordinary decompositions have been produced by the agency of the sun. The most energetic reducing agents which we know are required in the hands of chemists to produce some of these effects. When made red hot, the vapour of phosphorus and also potassium will accomplish the deoxydation of carbonic acid. Plants can do the same at 50° Fah. very readily.

222. If water be exposed to the light in glass vessels, common observation assures us that it never undergoes decomposition. In the same manner carbonic acid in a jar remains without exhibiting any change. We shall soon discover why, under these circumstances, the powerful reducing agency of light is not called into action.

223. We have seen that the appearance of green matter in water, and the production of chlorophyl in leaves, are the same phenomenon. In the largest trees all the solid matter found in their branches, stems, bark, and various other parts, was originally fabricated in the leaves, for it was in them that carbonic acid was decomposed and its solid material fixed. Once thus introduced into the interior of the vegetable system, it passes through a multitude of changes; from the descending sap the different compounds are removed, and lodged in their proper places. It is in this way that the starch, sugar, gum, woody fibre, fibrine, albumen, caseine, essential oils, resins, &c., are all produced. First one and then another change is impressed on the descending juice, and first one and then another special compound is removed from it; but the point from which all this cycle of changes begins may be traced back into the leaf, to the decomposition of carbonic acid and production of chlorophyl, under the influence of light.

224. A ray of light, as is well known (137), contains, under ordinary circumstances, several distinct principles. 1st. A principle which impresses the organ of vision with a specific sensation, and which is therefore spoken of explicitly as LIGHT; of this there are several modifications: one which produces in our eyes the sensation of a red colour; another, a yellow; a third, a blue; and these, conjoined in proper proportions, give rise to a white. 2d. A principle of radiant heat, the well-known characters of which are the power of producing expansion in bodies, of communicating a feeling of warmth; in this the phenomena of coloration have been traced as in the former case. 3d. A principle of chemical force, which seems to be intimately associated with light, but differs from that principle in wanting the power of affecting our organs of vision. From this intimate association with light, I have suggested for this principle the name of TITHONIC RAYS, in allusion to the fable of Tithonus and Aurora. In this, as in the former, the quality of coloration has already been traced. 4th. A principle of phosphorescence, which has the distinctive character of causing certain substances, when submitted to its influence, to shine for a short time after with a transient light; this principle also is invisible to the eye, but differs from the tithonic rays in the fact that, under certain circumstances, transparent glass is opaque to it.

225. Of these constituents, visible and invisible, in the solar beam, which is it that has charge of that digestive function of plants which we are now considering? Is it the light or heat, the tithonic or the phosphorescent ray?

226. Several years ago, experiments were made by SENNEBIER for the purpose of determining this question; his results seemed to show that the violet rays contained the active force. More recently, in France, M. MORREN commenced an inquiry of a similar kind, and came to the conclusion that the luminous rays, and more especially the yellow, were the cause of the phenomenon. In 1836, Dr. DAUBENY published, in the Transactions of the Royal Society, a very extensive series of experiments, the result of which went to show that the leaves of plants decomposed carbonic acid, and turned green most rapidly in the yellow ray, the other colours, orange, green, red, blue, indigo, and violet, producing the same effect, more and more slowly in proportion as their illuminating power was less. The plan which was followed by these different chemists

was to cause the plants to grow or to carry forward their decomposing action under the influence of light which has passed through glass stained with the different colours Thus, if young plants are made to grow beneath a shade of lemon-yellow glass, in a few days it is seen that such light does not exert a destructive agency, but they grow vigorously; and if, simultaneously, comparative trials are made with other shades of different colours, such as red, blue, green, &c., an estimate may be obtained of the comparative effects of rays of these different colours.

227. But it is well known that lights thus produced are not monochromatic, but contain a variety of different coloured rays; for when the ray which passes through them is examined by a prism, it is always dispersed into a variety of different colours. Thus, the light which passes through the ordinary variety of blue glass, analyzed in this way, is found to contain red, yellow, and an abundance of indigo and violet, although it looks of a pure blue tint. The blue light which passes it is therefore not a simple, but a composite blue, made up of many other colours, the blue predominating. And as this observation applies to almost all sorts of coloured media, their colours being compound and not simple, it is obvious that when we make experiments with them we shall be liable to be led into error, unless we determine what rays they actually transmit. Accordingly, Dr. Daubeny determined this point in the case of each glass and coloured medium that he used.

228. About the same time that these experiments were made in England, I commenced some of the same kind in the south of Virginia (Ap., 413, 497, 506, 515–517), where, during the summer season, the light is extremely brilliant. These served to show that under a yellow solution, such as the bichromate of potash affords when dissolved in water, the leaves of plants turned green, and the decomposition of carbonic acid gas was effected.

229. Soon after this the British Association for the Advancement of Science, in consequence of some results which had been obtained in England, which seemed to countenance the opinion of Sennebier, that the violet rays direct the function of digestion, appointed a committee to examine this matter at their expense. A number of experiments were consequently made, very much in the same manner as those which had been previously conducted by Dr. Daubeny; these seemed, however, to be at variance with his conclusions, and showed, that while the violet tithonic ray is the active agent which controls the process, the yellow ray of light is exceedingly injurious.

230. But it is now known that these results, like those of Sennebier, are erroneous, and that, as was first discovered by Morren in France, and by Daubeny in England, it is yellow LIGHT which directs the digestion of plants; that leaves can be greened, and oxygen given off, and carbon fixed by light that has passed through a solution of bichromate of potash, or through yellow glass (Ap., 784–792).

231. The proper mode of conducting these experiments is to employ the solar spectrum itself, a process which, it will be seen (Ap., Ch. XV.), was first resorted to in New-York. By some appropriate optical mechanism, a ray of the sun is transmitted into a dark room through a circular hole in one of the shutters, and kept motionless for several hours, although the sun may have apparently moved a considerable distance

over the sky. This ray (*a b, fig.* 122) is to be intercepted by a glass prism, *c*, which disperses it into its different coloured beams. And now, having provided a set of glass tubes filled with spring water, or, rather, water holding carbonic acid gas in solution, and in each placed the same number of leaves of grass or of some other plant, so that each tube may be as nearly like all the others as may be, these tubes, inserted in a small pneumatic trough, *c d*, are to be set in the spectrum of light, in its different coloured spaces, one in the red, one in the orange, one in the yellow, &c. Care also should be taken to exclude all extraneous light, so that that which causes any action among the leaves may be derived from the ray which comes in through the shutter only. Very soon, if the sky is clear and the sun brilliant, the phenomenon begins. In the tube which is in the most luminous part of the yellow ray small bubbles are evolved, and these rising to the top of that tube, there collect, so that after the lapse of a few hours a sufficient quantity may be gathered for measurement and analysis. The tubes that are in the orange and green lights simultaneously go into action, and when the sky possesses an intense brilliancy, they will even approach in the rapidity of their action to the maximum yellow. A few bubbles also make their appearance in the blue, but after an exposure of many hours, if scrupulous care is taken to shut out all extraneous light, no action whatever is perceptible in the extreme violet, where SENNEBIER supposed the decomposing force to be situated. From these things we therefore gather, that it is in the yellow light that the power controlling the function of digestion of plants is to be found, the other coloured beams, orange, green, red, blue, &c., following in the order of their illuminating power.

232. This prismatic experiment is one of the most beautiful objects which organic chemistry can offer, carried on in a chamber which would be totally dark were it not for the intensely coloured curves which are cast upon the walls by reflexion from the tubes, curves which often are many yards in length, indicating by their gaudy tints and brilliancy the intensity of the sun's light. The tubes and the vegetable leaves glow with the colours in which they are immersed. Meantime, the most interesting phenomenon which can be witnessed is silently going forward; dead and inanimate matter is, under the influence of the plastic beam, putting on the form of organization and life. Oxygen and nitrogen gases are exhaling, chlorophyl, and gum, and sugar, fibrine, and albumen are coming into existence. These are compounds which, under ordinary circumstances, are destined to be used as the food, and form part of the bodies of animals. For it is from atmospheric air, as organic chemistry shows, that plants spring, condensed out of it, as it were, by the agency of the solar beam; to the same source also it is that gradually during life, and totally after death, the parts of animals hasten to return.

233. A little reflection shows the great advantages which this mode of experimenting possesses. In the white light of the solar beam there is a fixed proportion of each of the component colours, and when such a beam is dispersed by prismatic action, and is simultaneously received upon vegetable leaves, we, in effect, measure out to them similar quantities of the different coloured rays, and observe the resulting action. When pieces of glass are used, as in the former experiment (226), a great deal depends on their

thickness. It is easy to conceive, that although in the solar spectrum the yellow ray is vastly more luminous than the blue, if glasses or coloured media were used in experiments of any kind, results of precisely an opposite nature might be obtained. We can imagine a piece of yellow glass so thick as actually to transmit less light than a thinner piece of the blue. With glasses and absorbent media, therefore, not only must the nature of transmitted light be discovered, but thickness must also be taken into account.

234. Suppose, as an illustration, plants were made to grow, or to decompose carbonic acid, under a thin piece of blue cobalt glass, such as that of which finger-glasses are made, and the result compared with what was occurring with another set of plants in action under a thick or deep yellow glass. It might turn out that the former would vegetate more vigorously, and fix more carbon, and produce more chlorophyl than the latter, because more light actually fell upon them, though the medium through which it came was blue. We must not forget that the mode of action in producing chemical decomposition, and the mode of action in producing vision through such an optical contrivance as an eye, proceed upon different principles. In the action of the eye time does not enter as an element (Ap., 536). In the decompositions produced by radiant matter, or by light, it does. A faintly luminous object does not become brighter and brighter as we continue to look steadfastly at it; it has assumed its maximum of brilliancy at the first glance. But a faintly luminous beam, falling upon leaves of plants, or upon any changeable compound, continues to produce an increasing effect as time passes on. In our experiment, more and more carbonic acid is decomposed, and more and more oxygen set free as the time is prolonged. And so when absorbent media, as stained glass, are used, the final effect is dependant on the total amount of light that has been furnished; and hence the turbidity, or thickness, or partial opacity of those media must be taken into account, as much as their colour-giving relations. In the prismatic experiment this source of disturbance does not occur.

CHAPTER VII.

ON THE VARIOUS IMPONDERABLE AGENTS EXISTING IN THE DIFFERENT REGIONS OF THE SPECTRUM.

235. From the preceding chapter, we have arrived at the conclusion that the imponderable principle which directs the digestion of plants is found at a maximum in the yellow space of the spectrum, or accompanies yellow light.

236. But it has been stated (224) that there are no less than three other principles

coexisting in solar light, tithonic, calorific, and prosphorescent rays; for the two former the phenomenon of coloration has been distinctly traced, as will appear in a subsequent chapter; for the latter, analogy would lead us to suppose that the same thing holds.

237. In yellow light, such as can effect the decomposition of carbonic acid, a variety of other principles exist—yellow tithonic rays, yellow calorific rays, and perhaps yellow phosphorescent rays. Prismatic experiments (Ap., 790) can only indicate the refrangibility of the ray which produces a given effect. Experiments by absorbent media are finally required to point out its exact nature.

238. In order that the reader may possess clear views of the evidence which is to be brought forward, showing that it is yellow LIGHT, and not tithonic, calorific, or phosphorescent yellow rays which superintend the formation of organic molecules out of inorganic matter, it is necessary to set forth in a more prominent way the distinctive characteristics which appertain to each of these principles.

239. Of the calorific rays, or rays of heat, nothing, however, need here be said. Their physical independence of light, and separate existence, were completely established by Melloni (170), who also discovered in them modifications analogous to coloration.

240. The existence of the tithonic rays was first ascertained during the last century. They passed in the books under the name of chemical rays—a name still continued by many chemists. As a multitude of phenomena with which they are connected became known, it has been found absolutely essential to give to them a specific name, such as can be moulded conveniently for the purposes of science, and combine readily to form those different words which are necessary to give names to instruments in which they are involved, or to phenomena with which they are connected—a name which, however, involves no hypothetical idea of their nature or action.

241. The most striking fact in connexion with these rays is their constant association with light. Though the two principles are separable from each other by artificial processes, yet, under natural circumstances, they always occur together. In sunlight, in gas-flames, from candles, and even in the moonbeams, in which no heat is found, the chemical and luminous rays exist together. They undergo reflexion, refraction, polarization, and interference, apparently under the very same laws. But while light can affect the eye, and bring us into relation with all the phenomena of the material world, these rays fail to affect our organs of vision—they are invisible.

242. In view of this constant association of light and the chemical rays, and to express the idea of their near alliance to each other, and as the wants of science imperatively called for a specific designation, the name of tithonic rays has been suggested for them. It is drawn from the fable of Aurora and Tithonus. This name forms compound words with facility. It does not involve us in any speculative considerations, but merely points out the fact that the principle to which it is given is almost always associated with light. As has been just stated, all the mechanical laws which regulate the reflexion, transmission, polarization, and interference of light, obtain also for this.

243. The mathematical theory of light is based upon the postulate of undulations taking place in an ethereal medium. This theory, like the theory of universal gravi-

tation, possesses an abundant internal evidence of truth. In our times extensive and important applications have been made of it, so that it now includes an explanation of all the phenomena of reflexion, refraction, polarization, double refraction, interference. As yet, it has furnished no clear account of the phenomena of absorption, the very phenomena which are at the basis of all physiological and chemical facts in their relations to luminous agency. As we shall presently prove, the decomposition of carbonic acid and the function of digestion in plants depend on the absorption of light. Nor is there yet included in it any representation of the various phenomena of heat. In the course of years, these things will probably be added, and one great generalization embrace all the phenomena of the imponderable agents. But the purposes of science require, until that event takes place, that we should continue to speak of the various imponderable principles as though they were different agents, and treat them as though they were separate existences.

244. The first decisive evidence brought forward to establish the physical independence of the heat and light of the sunbeam, as has been stated (165), was derived from the experiments of Sir W. Herschel, the illustrious astronomer. He found that, when a beam of light is dispersed by a prism, and the resulting spectrum examined, by placing thermometers in its different coloured spaces, the most luminous rays are not the hottest, the maximum temperature occurring, not in the yellow, but in the red ray; and even out of the red ray, and where the eye could detect no light whatever, heat was present, for the thermometer there rose rapidly. Starting from these experiments, Melloni added farther proof by showing that transparency for light is not necessarily transparency for heat; that there are certain media more or less opaque to one of these imponderables, and more or less transparent to the other, and that, by appropriate combinations, media can be obtained which will allow light to pass them with very little diminution of its intensity, but which stop the heat almost entirely.

245. In the dark rays which come from a vessel of hot water, we have radiant heat without light; in the moonbeams we have, on the contrary, light without heat.

246. By a series of experiments of a similar kind, the physical independence of the tithonic rays has also been established. During the last century, it was observed by Scheele, that these rays occur abundantly beyond the violet extremity of the spectrum, where the eye can discern no trace of light, an observation essentially of the same kind as that made by Sir W. Herschel for radiant heat. In consequence of this discovery, the occurrence of invisible rays was at once assumed, and, without any inquiry as to their nature, their existence became an admitted fact in science.

247. It does not appear, however, that any clear views were entertained as to the precise character of these dark rays. Writers on optics spoke of them indifferently under the name of violet rays, chemical rays, deoxydizing rays, and invisible rays. Of all the benefits which can be conferred on an infant science, those arising from establishing clear, bold, prominent, decisive views of its fundamental agencies and their actions are by far the most valuable, for they fasten the attention forcibly. In all progressive sciences, each epoch of evolution, for sciences advance forward by starts, is traceable to the announcement of some clear and vivid idea.

248. As we have said, the existence of these dark rays beyond the violet end of the spectrum was established during the last century. For, when a solar spectrum is made to fall on a piece of paper covered over with white chloride of silver, that compound turns black, the colour changing where blue, indigo, and violet rays fall; the change also extending over those spaces which are outside of the violet ray, and where there is no light. In 1836, by using bromide of silver instead of chloride, I found that these deoxydizing rays extended out of the blue light down towards the yellow (Ap., 445); these experiments were published in 1837.

249. From a variety of evidence, obtained about the same time, it appeared that when absorptive media were used, the rays of light could be deprived of much of their chemical power. Thus, on passing a ray through a solution of bichromate of potash (Ap., 410), it became almost inactive, refusing to produce any change on sensitive paper; a similar result following on passing it through Dalton's solution of the quadrosulphuret of lime, and a variety of other media (Ap., 410–509). At that early period in photographical investigations, when as yet no other chemist was engaged in these pursuits, two important facts were published: 1st. The physical independence over a great part of the spectrum of light and the tithonic rays (Ap., 386). 2d. That the tithonic rays exhibit undoubted tokens of modifications answering to colours in light (Ap., 384, 433, 506). This is the foundation of the doctrine now more completely unfolded in this work, under the designation of "*The Theory of Ideal Coloration of the Tithonic Rays.*"

250. From their constant association with light, it is difficult to give that clear evidence of the physical independence of the tithonic rays which may be given in the case of heat. Much weight may, nevertheless, be laid upon the circumstance that, by prismatic action, these rays can be proved to exist beyond the extreme violet region, and even beyond that ray which Sir J. Herschel designates lavender. Evidence of the same kind is derived from the partial action of absorbent solutions and media, which act differently on each of these classes of rays. From observations of this kind tables have been formed, with a view of proving this point (Ap., 673). In the Transactions of the Royal Society, Sir John Herschel has given some of the same sort (*Phil. Trans.*, 1840). My results were published in 1842.

251. At one time much stress was laid on the power which certain media, such as bichromate of potash and quadrosulphuret of lime, possess in depriving a ray of its chemical force. This argument is still resorted to by inaccurate writers, as affording a very popular and palpable proof that light and the tithonic rays are different existences. "If," it is observed, "a ray of light is passed through a solution of quadrosulphuret of lime, and is then received on paper covered with chloride of silver, it is found that the light alone has gone through, and the chemical rays have been absorbed. The one, therefore, exists independent of the other." When I first observed the power of these yellow solutions, this was the conclusion I came to (Ap., 410, 511). But the theory of ideal coloration, presently to be explained, deprives this argument of much of its force; for the rays that are transmitted by such a solution are yellow rays of light, and yellow tithonic rays; in these the chloride of silver does not change, its decomposition being brought about by the blue tithonic rays which have been absorbed along

with the blue light. Tithonic rays are present in a beam which has passed through quadrosulphuret of lime, but chloride of silver cannot detect them.

252. An accurate examination of the *quantity* of light and the *quantity* of tithonic rays passing through given solutions has, however, restored this argument to its pristine force. Thus, it is proved, that of different media which have been tried, some transmit more of the luminous and some more of the tithonic rays (AP., CH. XVIII.); that the rate of transparency for one is often totally different from the rate of transparency for the other; and although we have not yet been so fortunate as to discover any given medium which is opaque to one of these principles and transparent to the other, as has been done in the case of light, their physical independence is just as certain as though such a medium was known.

253. After commenting in the preceding paragraph on the weak point in the argument drawn from absorbent media, it ought not to be left without showing how experiments of that class may be rigorously used as proofs of the correctness of the views given in 1837. It only requires that the sensitive paper or surface used should be of such a character as to be affected by all the tithonic rays in the spectrum, irrespective of their order of refrangibility or ideal coloration. Such a substance is the bromide of silver. Sir J. HERSCHEL has used it with a similar object in view (*Phil. Trans.*, 1840, p. 38). Suppose, therefore, we allow a solar spectrum to fall upon such a changeable surface, after having passed through a yellow absorbent medium, as the bichromate of potassa. The eye at once informs us that very little of the yellow and orange light is lost, but we must keep the surface for a long time exposed before the yellow and orange tithonic rays will have produced such a change as they would have produced in a few moments if the bichromate had not intervened. In this case the experiment is a fair one, and the deduction it gives holds good, because bromide of silver is easily decomposed by the tithonic yellow ray. The bichromate of potash, therefore, transmits yellow and orange rays of LIGHT copiously, but it transmits the corresponding yellow and orange tithonic ray to a far less extent. The two principles are therefore distinct.

254. That there is nothing unphilosophical in supposing that an invisible principle such as that of which we are speaking should exist in solar light, is shown by the analogy of radiant heat, a principle equally invisible to our eyes, but of which the existence is palpable enough to our other organs of sense. In a dark room we are utterly unable to see a vessel of hot water, but its calorific emanations are plain to the hand, even at a considerable distance. In like manner, this analogy is supported by the recent discovery of BECQUEREL. For a long time it has been known that there are certain bodies, such as calcined oyster shells, which shine in darkness after a brief exposure to the light. A hundred years ago it was discovered that the transient light of an electric spark is sufficient to awaken the dormant glow of these bodies. Now BECQUEREL has shown, that to the rays which thus issue from an electric spark, and cause this wonderful phenomenon, *glass is opaque*, that light can pass through glass, but the phosphorescent rays cannot. They also are invisible to the eye.

255. It will be seen, by referring to CH. XII. of the AP., that there are certain phe-

nomena which may be explained on the supposition that the invisible tithonic rays escape by radiation from bodies which have been impressed by them, those bodies simultaneously reverting to their original condition. These results, and similar ones, have of late years attracted much of the attention of experimental philosophers, but the inquiries involved are beset by numerous difficulties (AP., 708, &c.). There is not, however, anything impossible, or even unlikely, in this secondary radiation. A phenomenon of exactly the same kind is visible to the eye in the case of the phosphorescent rays, when the glow of light by radiation escapes away from calcined oyster shells after they have been illuminated by an electric spark.

256. Dark, invisible rays thus exist in the sunlight, and carry on a variety of functions, and control a variety of phenomena. Of solar principles, four different kinds have been traced: rays of light, of heat, tithonic, and phosphorescent rays. The two former are admitted to constitute recognised imponderable principles. *What are the latter two?*

257. In the Philosophical Magazine (see AP., CH. XIII.) I have some years ago brought forward the doctrine, that we are compelled to enlarge our catalogue of imponderable principles, and include these tithonic rays in it. More recently I have offered similar arguments in favour of the phosphorescent rays (AP., CH. XVIII.).

258. This brings me to offer some remarks on the opinion expressed by M. BECQUEREL, that the phenomena now under discussion are due to the qualities of the receiving surfaces, and not to agents intrinsically different, coexisting in the solar beam. That the same *beam of light*, falling on sulphuret of lime, causes it to phosphoresce; on chloride of silver, blackens it; on the retina, gives rise to the phenomena of vision and colour; on a piece of black cloth, causes it to become warm. This opinion seems to be surrounded with insurmountable difficulties, and, if admitted, would disturb some of the best-established truths of science.

259. No one can feel more strongly the absurdity of supposing that Nature has created between forty and fifty elementary ponderable substances, all possessed of metallic characters, and all so nearly alike that even a chemist is often puzzled to distinguish them from one another. No one, upon satisfactory proof, would more willingly go back to the alchemical doctrine in relation to these matters; but so long as the evidence on the constitution of these bodies rests where it does, the laws of chemistry compel us to admit them to be simple and undecompounded. And, just in the same way that I am willing to admit the existence of forty different simple metals, so, upon similar evidence, I am free to admit the existence of fifty different imponderable agents, if need be. Is there anything which should lead us to suppose that the imponderables are constituted by Nature on a plan that is elaborately simple, and the ponderables on one that is elaborately complex? That the former are all modifications of one primordial ether, and the latter intrinsically different bodies, more than a quarter of a hundred of which have been discovered during the present century?

260. Before admitting the correctness of the hypothesis of M. BECQUEREL, that the agent under consideration is one and indivisible, and that all the phenomena we discover are due to the receiving surfaces, and that there are as many spectra as there are

substances in nature, each giving its own manifestations when exposed to the sun's ray, we should make inquiries like the following: How is it that a piece of black cloth exposed to the moonbeams does not become warm? How is it that a cannister of hot water is not luminous to the eye? In the rays that come from the moon, and those that are emitted by warm water, is there no intrinsic difference, or does the phenomenon depend on the receiving surface alone? What becomes of the beautiful experiments of Melloni, on the physical independence of light and heat, since these are mainly founded on the fact, that by the use of absorbent media we can separate one from the other? How is it that the rays of an electric spark, passing through quartz, can make the Bolognian stone phosphoresce, but passing through glass, equally transparent and equally colourless, can do no such thing? The receiving surface is the same in both cases, and, as far as human eyesight can discover, the light that comes through the glass is as pure and unaltered as the light that came through the quartz, but the results are diametrically opposed. And is it not more consonant to reason to suppose that the glass was opaque, or impervious to some agent existing in that beam, which freely passed the quartz—opaque to it, but transparent both to light and the tithonic rays?

261. We might multiply cases like these, and give similar arguments from chemical changes on sensitive surfaces; but the instances already cited seem abundantly sufficient to overturn the hypothesis in question. Before it can be admitted, it must give a reason why the retina is not affected with the sensation of vision when rays from hot water fall on the eye, why a thermometer will not rise when placed in the moonshine, why sulphuret of lime or barytes will not phosphoresce when covered with a piece of glass.

262. We are thus forced to admit that rays of light, rays of heat, tithonic rays, phosphoric rays, and probably many other radiant forms, have an independent existence, and that they can be separated, by proper processes, from each other.

263. It must, however, be understood, that the conclusion here arrived at essentially depends on the following facts: 1st. The constant visibility of light. 2d. The uniformity of the action which heat exhibits in expanding bodies. If the progress of science should require us to admit that there can exist light which impresses our eyes with a sensation of darkness, or heat which can neither raise a thermometer nor produce the sensation of warmth, then the force of the foregoing arguments will be essentially affected. Such extensive changes in the universal acceptation of words will, unquestionably, be very slowly received.

CHAPTER VIII.

IT IS YELLOW LIGHT WHICH CONTROLS THE PROCESS OF DIGESTION IN PLANTS.

CONTENTS: *Examination as to which of the Principles mentioned in the preceding Chapter is engaged in the Decomposition of Carbonic Acid.—It is not Radiant Heat.—Melloni on the Ideal Coloration of Heat.—Analogies in the Case of Light.—Herschel's Results.*

It is not the Tithonic Ray.—Maximum of Decomposing Action for Carbonic Acid and Carbonaceous Compounds, like the Retina, is in the Yellow Ray.—Hence the Maximum of Visible Illumination coincides therewith.

264. In the last chapter, and in the APPENDIX, CHAPS. XIII., XV., and XVIII., we have given proofs of the separate existence of a number of independent imponderable principles in the solar beams, of which only one produces a specific effect upon the eye, the others being wholly invisible, and known to us by the chemical or mechanical effects they produce.

265. When, therefore, a beam of light falls upon a prism, and is decomposed by it, and the resulting colours are received upon a screen so as to give rise to a spectrum, such as that represented in the frontispiece, in each portion of that spectrum these different imponderable principles are present. For example, in the region between the lines A and C there are red rays of light, red rays of heat, and red tithonic rays. They are mingled together there, fortuitously, through the optical action of the prism. Their existence is perfectly separate and independent, and any one of them may be removed by proper processes, and the others left.

266. Confining our attention now to the yellow portion of the spectrum, in which region the decomposition of carbonic acid takes place, it is obvious, upon these principles, that there are coexisting there yellow light, yellow heat, and yellow tithonic rays. It remains for us to inquire to which of these three principles the decomposition of carbonic acid and the production of green matter is due. The phosphorescent rays may be left out of the discussion, though these, with the other three classes, are spread all over the spectrum.

267. First let us ascertain whether radiant heat, generally, has the quality of producing decomposition. To rays coming from a brightly-burning fire, I exposed some vegetable leaves in water holding carbonic acid gas in solution, and, to increase the effect, converged the calorific rays by a large metallic concave mirror. That no doubt might remain of the incapacity of heat to produce the phenomenon, the temperature of the water, under the influence of the radiant heat, was allowed to run up to 140° Fah., a much higher point than is ever attained under natural circumstances. But neither at low temperatures, nor at these elevated ones, did any visible decomposition

take place; showing, thus, that heat alone cannot cause the digestion of plants. Moreover, as is well known to chemists, carbonic acid gas may be passed through a tube that is white hot without giving the most remote appearances of decomposition.

268. The beautiful experiments of Melloni have proved, however, that rays of heat emitted by bodies at different temperatures vary in their constitution. At a low temperature, such as that of the human hand, the caloric emitted is of a high refrangibility, and possesses invisible violet coloration. As the heat rises, rays of a lower refrangibility are sent forth, so that if we examine the character of the rays coming from a series of bodies, the temperatures of which are successively higher and higher, as the hand, a vessel of boiling water, a red-hot iron, a gas flame, the refrangibilities become lower and lower; the radiant heat possessing a calorific tint, which successively descends through the colours of the spectrum, the violet, indigo, blue, green, yellow, &c. From the sun, the temperature of which, therefore, must be excessively high, radiant heat is emitted which occupies a region in the spectrum corresponding to the red rays, and even below that is found beyond the region where red light has ceased to be visible. In the sunbeam, therefore, rays of heat of every refrangibility and every colour are found; they occupy a space commencing with a region beyond the extreme violet, and, descending through the whole length of the spectrum, are found beneath its lowest extremity.

269. This is a phenomenon analogous to what we witness under similar circumstances in the case of light. When a lamp, the wick of which is placed very low, is first lighted, it burns with a violet-coloured flame, giving forth little heat, and possessing small illuminating power. By-and-by, as the combustion goes on, the colour passes through various shades of indigo, and presently becomes of a purer blue. If the wick is now elevated, and air more abundantly supplied, the light increases in brilliancy; and, if seen through a prism, all the colours begin to be perceptible from the violet to the yellow and orange. Lastly, if fed with oxygen gas, or consumed in one of the improved burners, the light assumes a beautiful whiteness, and, if dispersed by a prism, exhibits all the colours of the spectrum. It is to the presence of these that its whiteness is due, for white light contains all the coloured rays.

270. In these beautiful and perfect analogies, which may thus be traced between the phenomena of light and heat, there are some points which require to be considered in the case we have before us. As common observation assures us, rays of light of different refrangibilities excite in our eyes specific sensations; the most refrangible ray affects us with that sensation which produces in the mind the idea of a violet colour; the middle refrangible ray, a yellow; the lesser refrangible ray, a red. And now these impressions, thus passing along the optic nerve to the brain, originate in specific changes which are happening to the constitution of the retina, for this delicate expansion must, in the nature of things, be acted upon under the influence of the light, in order to give rise to a mental sensation. The different rays of light, each one for itself, operates in its own way and produces its proper result.

271. Considerations like these would, therefore, lead us to suppose that rays of heat of different invisible colours ought to have the property of producing specific changes.

To them all is apparently given a power of producing expansion in bodies, and to each one, probably, its own specific chemical powers. The experiments of Sir J. HERSCHEL seem already to give proof of this fact.

That the decomposition of carbonic acid by leaves is not due to yellow heat, may be proved by causing the active light to pass through a solution of bichromate of potash, which is of an orange-yellow colour. This ray, thus treated, appears to carry on the decomposition with nearly the same activity as the direct solar beam (AP., 788). In an experiment which I made, using it in a stratum of certain thickness, it seemed to transmit the yellow and orange light with very little loss; but acting more energetically on the calorific ray, it transmitted of it only ·26. Had this heat been the cause of the decomposition, the rapidity with which the action took place should have been proportionally reduced.

272. From such results, it is to be inferred that radiant heat generally, and the yellow rays of heat especially, do not produce the decomposition of carbonic acid gas in the structure of vegetable leaves.

273. This narrows the question down to the inquiry, whether it be the yellow ray of light or the yellow tithonic ray; for, as has been observed, the phosphorescent rays may be left out of the discussion.

274. Experiments conducted on the same principle, and nearly in the same way, with that already cited, serve to determine this point. If a beam which has passed through a solution of bichromate of potash retains its power, as we know by experiment, it remains for us to inquire whether that solution acts as feebly on the yellow tithonic ray as it does on the yellow ray of light. We have already shown (251) that this is not the case, for the tithonic ray undergoes an abundant absorption, its force being greatly diminished, so that bromide of silver, which is easily changed by the yellow tithonic ray, undergoes, in this disturbed and absorbed beam, a slow decomposition. In the same way, therefore, that we determine that it could not be radiant heat, we also determine against the tithonic rays. The decomposition of carbonic acid, the production of chlorophyl, and the greening of plants go on with great rapidity under that yellow solution, because, although it has absorbed, and therefore removed these imponderable agents, it allows the active light to pass with little or no diminution.

275. The observation made in 237 is now understood. Analysis by the prism serves only to point out in what particular region of the spectrum given phenomena are produced; it therefore narrows our discussions down within certain limits. By introducing, in addition, the action of absorbent media, we are enabled to point out, with a certain amount of precision, the exact agent which is involved. This use of absorbent media conjointly with prismatic analysis, which was introduced into these inquiries in 1837 (AP., CH. X.), may be expected continually to yield interesting results.

276. To the LIGHT, and more especially to the yellow light of the sun, we are to impute the production of this, the most interesting phenomenon of organic chemistry. Rays which come from artificial sources, such as lamps and gas flames, can also bring it about to a degree corresponding with their intensity.

277. Whether that peculiarity of light by which it gives to us the sensation of spe-

cific colours is involved, remains as yet undetermined; we cannot say whether the quality by which we are led to impute to a given beam a yellow colour is the same quality which is involved in this decomposition. Whether, in short, yellow light, because it is yellow light, produces this change. Would any other coloured ray, such as a blue, if its intensity were sufficiently elevated, produce the same result? For we can imagine a blue light so to be re-enforced as to possess the same intrinsic brilliancy or illuminating power as a yellow. Under such a change, would its decomposing action also be exalted?

278. Prismatic experiments serve to show (Ap., 782) that the rapidity of decomposition follows very closely the order of illuminating power. And this result affords an argument, imperfect and feeble it is true, that an affirmative answer will be hereafter given to that question.

279. But there are other reflections which naturally arise, and tend to an opposite result. There seems to be a general relation, though the details of it have not yet been traced, between rays of a particular refrangibility and ponderable substances of a particular kind. Thus, in the case of most of the salts of silver, the point of maximum action falls in the violet ray. In the same way the question naturally arises, Does the point for the maximum action on carbon compounds fall in the yellow space, and the yellow, for that reason, become the active ray in decomposing carbonic acid, and giving a green colour to leaves? Is it for this cause, also, that, received into the eye, the yellow ray impresses us with the greatest illuminating power? It would be a beautiful result of these researches to co-ordinate phenomena apparently so widely apart as the formation of chlorophyl in a leaf and the regulated destruction of the retina in the chamber of the human eye in producing the phenomena of vision. In nature there are many results which are apparently equally distinct, and which the progress of knowledge has shown are intimately allied. That to our organs of vision yellow light is the most brilliant, arises from the incidental circumstance that it is a carbonaceous compound of which the changing nervous expansion is constructed. Had it been possible for Nature to have formed a retina in which a salt of silver formed the basis, the maximum of brilliancy of light would have shifted, and the blues would have been among the brightest rays. Is it in the optical peculiarities of the carbon atom that all our ideas of harmony among colours and beauty of external objects have arisen?

280. Experimental science will probably before long trace a close connexion between the physical properties of atoms and the physical properties of light. It will show that molecules of a given weight can be moved most easily by ethereal waves of a given length, as a stretched string is thrown into vibration by atmospheric undulations of proper dimensions; that the transverse vibrations of the ethereal particles can agitate in a corresponding way ponderable atoms of a proper magnitude and constitution. We shall then have no difficulty in understanding how it was that among metallic substances, those first detected to be changed by light, such as silver, gold, mercury, lead, have all high atomic weights; and such as sodium and potassium, the atomic weights of which are low, appeared to be less changeable.

CHAPTER IX.

THEORY OF THE ABSORPTION OF THE TITHONIC RAYS AND LIGHT.

CONTENTS : *Estimate of the Extent and Power of the Solar Radiations.—Influence still exists in the Moonbeams.—Absorptive Action of Chlorine and Hydrogen.—Detithonization of the Ray and Tithonization of the Gaseous Mixture.—Curve and Law.—Deductions as to Latent Light and Definite Action.—Functions discharged by the Chlorine and Hydrogen respectively.*

281. IN pursuing our discussion of the phenomenon which we have under consideration—the digestion of plants—we have successively traced the source of action to the yellow region of the spectrum, and to the ray of LIGHT.

282. In what manner, then, does this light act? How does it come to pass that it can exert so great a force as to effect the reduction of carbonic acid in the cold? We have briefly seen what are the results it impresses on the forming vegetation, that the leaf turns green and oxygen is given off. What are the corresponding and contemporaneous changes which happen to the light? Action and reaction are always equal, and if a given beam can produce a result which demands the most energetic chemical force, it is reasonable to suppose that in doing so it undergoes itself a change.

283. These considerations show us that the question in what manner yellow light acts in controlling the function of digestion in plants, is not only exceedingly interesting in a physiological point of view, but also that it involves the whole theory of the action of radiant matter, whether it be of light, heat, or tithonicity, in producing chemical change.

284. There are few authors who have written on the action of light or the tithonic rays in producing chemical changes, who have not directly ascribed all those changes to absorption of the imponderable principle. The connexion between absorption and the production of these phenomena is clearly apparent. Still, however, in looking over what has been written, we find little precision in those views; instead of a distinct conception of a plain fact, we find only loose and imperfect ideas.

285. In animals, voluntary and involuntary motions are under the government of the nervous system. Each movement which is executed is attended with a corresponding consumption of organized matter, either in the muscular or nervous tissues, or both. The motions which my fingers are executing in writing these lines do not spring forth from nothing, but are the offspring of the destruction, in a regulated manner, of organized matter originally derived from food. How perfect, then, is the animal machine, which, fed from day to day by a small portion of carbonaceous matter, executes motions with an inconceivably small expenditure of material! How great, also, are the results which may arise from the return of those organized atoms to their pristine inorganic

state. Thrown out from the mechanism, after their office is over, they leave behind them marks of the changes through which they have passed, and of the facts to which they have given birth, and thus stand at last in connexion with events to which at first they were not apparently allied.

286. From the combustion of small quantities of carbon, we see, in improved steam-engines, how great an amount of force can be originated, and by the oxydation of a few grains of zinc in voltaic batteries, what surprising chemical results arise. From those more ordinary cases of changes accomplished by the action of light, which appear to be feeble and slowly produced, we should form the most erroneous opinions of the force of the sun rays. General considerations might lead us to know that the principle which has in charge the keeping up of the constitution of the atmosphere, and regulating the vital functions of plants, is of great intensity. Thus, I have found that the rays which are emitted from a common wax candle are superior in chemical force to the current which is evolved by a cell of Grove's battery, the most powerful of voltaic combinations known, for they could effect the recomposition of muriatic acid much faster than the battery could decompose it, and yet that battery was found competent to maintain a platina wire white hot, and, if the views of Dr. FARADAY are correct, was evolving more electricity than is developed by any thunder storm. If this is the case with a candle, what, then, shall we say of brilliant rays of the sun, which impinge on the earth on all sides?

287. That force, therefore, the mode of action of which we are now to discuss, is far from an insignificant power in Nature. In generality and intensity it rivals any that is known; in interest it is superior to them all, for it stands in connexion with organization and living things.

288. Even after having undergone that enormous reduction of intensity which must take place in reflexion from the surface of the moon, the solar rays still act with energy; for the moonshine produces all kinds of decompositions, acting like the sunbeam, though in a feeble way, being probably reduced to $\frac{1}{300000}$ part of its original power (AP., 545).

289. This force, thus affecting in a radiant form vegetable organizations, produces he results we are studying. The idea commonly entertained of its feebleness is utterly inaccurate and wrong.

290. In proceeding now with the more immediate object of this chapter, I shall follow the course of thought which has presented itself in my experiments. At the risk of dwelling somewhat in tedious detail, I shall also describe the different experiments from which the final arguments are drawn; and as it will be found that the rays of light and the tithonic rays are eventually under the government of the same laws, similar expressions including the phenomena of both, I shall commence with giving the theory of the absorption of the tithonic rays first, and then show how the same theory includes the phenomena of light. This leads naturally to a division of the subject under two heads.

I. Theory of absorption of the tithonic rays.

II. Theory of ideal coloration.

To the first of these the present chapter is devoted; the following chapter to the second.

291. When a beam of light has fallen on any changeable surface, such as a Daguerreotype plate, and is reflected by it, that beam will be found to have impressed a change on the sensitive surface, greater or less in amount, according to the period of its action. In effecting this it also suffers a change itself, and if received on a second similar sensitive surface, is found to have lost the quality of giving rise to the decomposition again. Two changes have, therefore, occurred, a change in the ponderable body, and a change in the incident beam. The particular experiments in proof of this fact are given in Ap., 595.

292. Again, let us take a second instance. As is shown in Ap., Ch. XVI., a mixture of chlorine and hydrogen in equal volumes undergoes combination by the influence of the rays of a lamp, and a rapid action, amounting to an explosion, by the brighter beams of the sun. As these gases can be obtained in a state of uniform purity, and their combination is attended with mechanical results, this forms a favourable case for a minute investigation. With one such clear case to guide us in our researches, we may fall back on it for illustrations, as new phenomena arise.

293. A mixture of chlorine and hydrogen, such as has been referred to, was placed in a vessel made of plate glass, having flat and parallel sides; it was 7 inches high, 2 broad, and 2·6 deep. It was so arranged on a small porcelain trough, that it could be used as a gas jar. The rays of an argand lamp, properly situated, were made to pass through it; they therefore went through a depth of the compound gases of 2·6 inches. In *fig.* 123, A is the lamp, so adjusted as to burn steadily, B the vessel containing the chlorine and hydrogen, C the porcelain trough, in which was placed a saturated solution of common salt, which acts on the chlorine slowly, and therefore allows us to make any necessary experiments without much change happening in the gases under trial. At D was placed a tithonometer (Ap., Ch. XVI.), to receive the rays from the lamp, after they had emerged from the chlorine and hydrogen.

294. Two separate phenomena were now apparent: first, the mixture of chlorine and hydrogen began to unite under the influence of the rays of the lamp; second, the rays which had passed through the mixture had lost very much of their chemical power. It was not totally extinct, but the tithonometer showed that it had undergone a very great diminution.

295. We see, therefore, that on its passage through a mixture of chlorine and hydrogen, the beam has become *detithonized.* Simultaneously, and in producing this result, we see that the sensitive mixture has become *tithonized.* The connexion and sequence of the phenomena are apparent. The beam has undergone a change itself in producing a given change in the ponderable matter. But this is the same conclusion that was furnished us by the rougher experiment with iodide of silver above quoted (Ap., 595).

296. At this stage of our inquiries, therefore, we have already fallen upon one of the leading features of the doctrine of absorption; for we perceive that, whenever tithonic rays produce a change on a sensitive mixture, they must necessarily undergo a change themselves, and become partially or perfectly detithonized.

297. From this point our inquiries naturally branch in two directions. First, The consideration of what happens to the substance which is thus in the act of being changed or tithonized. Second, What happens to the ray in undergoing its converse change, or being detithonized. These I shall discuss in succession.

298. *Phenomena of the Tithonization of Chlorine and Hydrogen.*—The tithonometer enables us to ascertain the leading phenomena in a very satisfactory manner. Its sensitive material being (AP., 838) the very mixture the properties of which we are considering, to determine the changes, and the rapidity of the changes which take place in that mixture, we are only required to place a tithonometer in the rays of a lamp, remove all external sources of disturbance, such as the action of radiant heat, &c. and note the results.

299. To carry out these views, I have employed the arrangement represented in *fig.* 124. A is an argand lamp, which, during the period of observation, burns with uniformity. In front of this, and at a distance of about two inches, an arrangement of double convex lenses, B, is placed. Beyond, at a distance of 7 inches, is a second convex lens, 3·5 inches focus. Between B and C, a metallic screen, E, is arranged, so that it can be easily removed or replaced, according as it is desired, to allow the rays of the lamp to fall upon the tithonometer, or to cut them off.

300. The mode of action of the lenses of this arrangement is, to give a uniform disc of light, M, on the sentient tube of the tithonometer. When a piece of white paper is placed so as to receive this, in front of the instrument, there is a circular disc, which is equally luminous all over. If this condition be not exactly fulfilled, the lamp or the lenses are to be moved and adjusted until the illumination is sensibly the same. We have then the sentient tube of the tithonometer plunged in an area of light which remains uniform in intensity during the period of our researches.

301. In this invariable disc of light we have to expose the mixture of chlorine and hydrogen, and mark on the scale of the tithonometer the progress of its union. This we do by noticing how many seconds elapse before the contraction arising from the production of muriatic acid begins, and then how many seconds elapse as the liquid in the index tube passes over each division.

302. In a particular experiment of this kind, the following numerical determinations were obtained:

303. On removing the screen E, and allowing the rays to fall on the sensitive mixture, first of all an expansion, amounting to half a degree upon the scale, was observed. In sixty seconds this expansion ceased.

304. The mixture now remained stationary, no apparent change going on in it. At length, after the close of 270 seconds, it was beginning to contract, and muriatic acid to form.

305. At the end of 45 seconds more, a contraction of half a degree had been accomplished; the dimensions of the mixture were therefore now the same as when the experiment first began; this half degree of contraction compensating for the half degree of expansion.

306. The number of seconds which elapsed as the liquid descended over the scale,

through contraction of the sentient gases, was now determined. These numbers are contained in the following table. In one column is given the number of each division; in the adjacent one, the period of contraction through it.

Spaces.	Time in Seconds.	Spaces.	Time in Seconds.
Expanded ½ degree in . . .	60	16	18
Movement commenced . . .	270	17	18
Reached ½ degree of expansion	45	18	18
1	55	19	18
2	40	20	18
3	28	21	17
4	27	22	17
5	25	23	18
6	23	24	17
7	22	25	18
8	20	26	18
9	20	27	17
10	21	28	16
11	20	29	16
12	20	30	16
13	20	31	16
14	19	32	16
15	19		

Let us now take these observations and project them, as is done in *fig.* 125, the amount of contraction representing the quantities of gases that have united being laid off on the axis of an abscissas, the times on the ordinates. But these times represent the number of rays which have fallen on the sentient mixture; consequently, the ordinates of that curve represent the quantities of tithonic rays, and the abscissas the corresponding chemical effects.

307. Inspection of the curve shows its peculiarities at once. We see that after the first preliminary expansion has taken place, expressed by that portion between *a* and *b*, for a certain space of time, although rays are constantly falling on the mixture and being absorbed, no visible effect is produced, there being neither expansion nor contraction, as is shown by *b c*. A certain space of time, amounting in this instance to 270 seconds, being now accomplished, contraction, from union of the gases, begins. In that portion of the curve *c d* which represents the progress of the phenomenon, the curvature is perpetually diminishing, and at *d* approaches sensibly to a straight line. From *d* to *e*, which includes the remainder of the observations, the line preserves its rectilinear character.

308. The study of the properties of this curve, or of the tabular numbers, serves to prove that when chlorine and hydrogen unite under the influence of the tithonic rays, there are four distinct periods of action.

1st. For a brief space the mixture expands.

2d. For a much longer period it then remains wholly stationary, neither expanding nor contracting, although the rays are constantly falling on it and it is absorbing them.

3. Contraction arising from the production of muriatic acid begins, commencing at first slowly, then more and more rapidly.

4th. And after that contraction has fairly set in, it goes on with uniformity, equal quantities of muriatic acid being produced in equal times by the action of equal quantities of the rays.

309. If, therefore, it is permitted us to generalize from this case of the action of rays on one of the most sensitive substances known, a mixture of chlorine and hydrogen, we

should assert that when a ray falls on a sensitive compound, its first effect is to produce an expansion. During a certain period, differing in different cases, no change whatever takes place, the ray being constantly absorbed, and not appearing to produce any visible effect. After a time, chemical action commences, at first more slowly, then with a determined and constant rate of rapidity, equal quantities of the rays now producing equal chemical effects.

310. I may here anticipate what will presently be proved, that this generalization can be sustained, and that, therefore, the conclusions which we have arrived at for this particular case, hold also for all others, not only in the cases of chemical action produced by the tithonic rays, but also of chemical action brought about by the rays of light.

311. Let us return again to the study of the curve, or of the numbers contained in the table. It is obvious that there are two portions of these which demand peculiar attention; they are embraced in the second and fourth epochs referred to in (308). As to the first and third, these are for the present of less interest.

312. What, then, is the interpretation we are to put upon that part of the curve which is between *b* and *c*? or, in other words, what is the interpretation we are to give of the fact, that when a sensitive compound is exposed to a given ray, it does not change all at once, but a certain period must elapse during which absorption is going forward, without any corresponding apparent effect ensuing, and that once accomplished, chemical change begins?

313. Is not this the same phenomenon which has been for a long time known in the case of radiant heat? When a ray of heat falls on a mass of ice at 32° Fah., in which a thermometer is imbedded, for a certain space of time no apparent rise of temperature takes place, but the radiation continuing long enough, a physical change is accomplished; the ice puts on a fluid form, and now the thermometer commences to ascend, equal quantities of heat producing, for a certain period, proportionally equal effects. Would not the table given in (306), or the curve projected in *fig.* 125, answer as well to express the phenomenon of the action of caloric upon ice, as of the tithonic rays on a mixture of chlorine and hydrogen?

314. It was from the study of that phenomenon in the case of ice that the doctrine of latent heat arose; and do not these things teach us that just as a calorific ray becomes latent under certain circumstances, so also does a tithonic ray, and, consequently, a ray of light? I regard the phenomenon of that pause which is seen before chlorine and hydrogen unite, and during which absorption is taking effect, as setting forth in a strong, and clear, and prominent manner, that as radiant heat may become latent, so also may tithonic rays, and also rays of light.

315. Let us, in the next place, direct our attention to the second branch of the curve, or to the fourth epoch, the changes of which are included between *d* and *e*. Rigorously speaking, this is not a straight line. It only makes a sensible approach to one. There are several causes which obviously interfere. When a given quantity of gaseous mixture is exposed to a radiant source, and the experiment we have been relating performed, it is obvious that, as it proceeds, the volume of gas so exposed steadily di-

minishes, and the effect of this must be apparently to diminish the force of the ray. Moreover, during the progress of the trial, muriatic acid gas accumulates, for its absorption does not instantaneously take place; the operation of this must be the reverse of the former, or apparently to increase the effect. To a certain extent, but not perfectly, these sources of error may be avoided, as I have attempted, and practically we may discuss the branch of the curve, *d e*, as though it were a straight line.

316. This being understood, the result which is before us leads us obviously to this important law, that for a given compound, equal quantities of tithonic rays, after the preliminary latent absorption is over, give rise to equal chemical effects.

317. In thus setting forth, in as prominent a manner as I am able, these two doctrines, 1st. Of the latent condition of the rays that are at first absorbed; and, 2d. Of the definite chemical action of those that are subsequently, I am again urging the same doctrine which three years ago (Ap., 595) I attempted to establish for iodide of silver.

318. In passing, it may be observed, that one of the greatest practical difficulties in the art of photography, more especially in taking portraits from life, is connected with this matter of preliminary latent absorption. Artists know well, that to obtain a perfect result is the exception, and an indifferent one is the rule. It is, indeed, rare that the relation of light and shadow is perfectly observed; the high lights most commonly come out unduly, the feebler lights are more slowly evolved, and very often never come out at all. This arises from the circumstance that the sensitive surface does not begin to change uniformly from the first instant of exposure, but the preliminary latent stage of absorption has to be gone through; with high lights and a brilliant illumination, that period is passed over in an instant, and the second entered upon; but the feebler lights have to expend themselves for a long while in passing through this stage, and while the others have carried on their operation almost to completion, these have not been able to leave a sensible trace of action. Theoretically, the remedy for this difficulty would be, by a brief exposure to a transitory or dim light, to pass the sensitive surface uniformly through its preliminary stage.

319. These, therefore, are the prominent phenomena which are exhibited by a mixture of chlorine and hydrogen, a latent preliminary absorption, and a subsequent definite chemical action. We have observed them also in iodide of silver (317), and in various other compounds.

320. Let us direct our attention, in the next place, to what has happened to the ray. We have already seen (295) that when, through a gaseous sensitive mixture, the beams from a lamp are suffered to pass, and fall on the tithonometer, they are found to have lost much of their chemical force. The beam has therefore become detithonized.

(*a*.) The vessel described (293) was filled with atmospheric air over the trough, and the chemical force of the ray passing through it from the lamp (*fig.* 123) was determined. It was measured by the period required to cause the index to descend through one division, and represented by 12 seconds.

(*b*.) The vessel was now half filled with chlorine, derived from a mixture of muriatic acid and peroxide of manganese, and the chemical force of the ray, after passing through it, determined as before; it now was represented by $25\frac{1}{2}$ seconds.

(*c.*) To the chlorine an equal volume of hydrogen was now added, the vessel being, consequently, full of the united mixture. The force of the ray was again measured, and found to be represented by 19 seconds.

(*d.*) Lastly, the first (*a*) of these preceding measures was determined again, with a view of ascertaining whether the intensity of the lamp had declined, or the apparatus remained in its first condition. It gave again 12 seconds.

321. Let us group these four results together, representing thus the intensity of the beam by the time it requires to produce a given effect:

A beam through the glass vessel	12	seconds.
Chlorine	25·5	"
Chlorine and hydrogen	19	"
Atmospheric air	12	"

We therefore gather from this, that the addition of hydrogen to chlorine, far from increasing its absorptive power, actually diminishes it. That in the case before us, where to a given volume of chlorine an equal volume of hydrogen has been added, the absorptive power is diminished to one half.

322. We farther see, that the action of the beam is expended primarily on the chlorine, giving to it a disposition to go into union with hydrogen, and that the functions discharged by the chlorine and hydrogen respectively are wholly different.

323. The chemical forces of the ray are easily deduced from the foregoing measures, in which the times are given, for it is obvious that they are inversely proportional to those times.

324. The second and third experiments (*b*) and (*c*) may, without sensible error, be taken as representing the activity of the ray in vacuo, for, as will be seen, upon principles hereafter given, a ray which has passed through atmospheric air has not undergone any absorptive action, and therefore does not differ from one which has passed a vacuum. Consequently, those measures give us the effect of chlorine, and of chlorine and hydrogen, compared with a vacuum. The absorptive action of the glasses is common to all the experiments, and may therefore be left out of the final estimate. The difference of the resulting numbers in *b* and *c*, from the probable numbers 25·5 and 18·7, may be accounted for from the disturbing causes which are encountered, such as the constant solution of chlorine by the salt water, &c.

325. When, therefore, a ray of light falls upon this changeable compound, chlorine and hydrogen, the primary action takes place upon the chlorine, which becomes tithonized, or has a disposition given to it to go into union with the hydrogen; the latter gas appears to be passive, so far as the ray is concerned. In the mean time, the ray itself becomes changed, undergoing absorption action, and being detithonized.

CHAPTER X.

THEORY OF IDEAL COLORATION.

CONTENTS: *Former Observations on Colours in the Chemical Rays.—Nomenclature derived from it.—Case of the Chrysotype.—Case of Bichromate of Potash.—Laws deduced.—Control of Optical Forces over Chemical Effects.—Application to Spectrum Stains.—Herschel's Law for Light.—Explanation of Variable Effects in Films of different Thickness.—Mode of Action of the Tithonic Rays.*

326. IN the year 1837, while the study of the chemical agencies of light was yet in its infancy, from phenomena connected with the decomposition of carbonic acid, the synthesis of chlorine and hydrogen, and the decomposition of chloride of silver, I came to the conclusion that there were among the chemical rays intrinsic differences, of the same order as the differences in colour among the rays of light, and that for those rays the doctrine of invisible coloration would have to be admitted (AP., CH. X.).

327. More recently, M. MELLONI, to whom science is indebted for originating and developing this beautiful thought, in the case of rays of heat, has independently come to the same conclusion. So intimately is this idea bound up with the explanation of the phenomena, that, in the case of radiant heat, that eminent philosopher proposes to use it as the foundation for the whole science, and for its nomenclature.—(*Taylor's Sc. Memoirs*, vol. iii., p. 12.)

328. In what follows, I shall not attempt to introduce the doctrine in an analytical way, or to trace the arguments and experiments which lead to its conclusions; but, assuming it at once as true, show with what facility and ease it may be brought to explain a number of remarkable phenomena. Calling to mind the facts and views which have been given in the preceding chapter, and more especially to the general fact, that whenever a ray produces a chemical effect it undergoes a change, the sensitive or changeable medium absorbing some one or more of its constituents, it remains now, in addition to the considerations there given respecting the characters of the rays, to introduce another element—the element of variable refrangibility.

329. Asserting, therefore, that for the chemical rays there exist peculiarities analogous to the different colours of light—peculiarities which, by reason of the invisibility of those rays, are known to us only by certain chemical phenomena—it remains, in bringing forward this doctrine with clearness and precision, to adopt some provisional nomenclature, and, for want of a better, one founded on those terms heretofore introduced (242) will serve our present purposes.

330. In developing the doctrine of ideal coloration applied to the tithonic rays, I shall therefore resort to what appears to be the simplest rule, and designate rays of different orders of refrangibility by the same nomenclature which has been used for light: thus we shall have

Tithonic red.
Tithonic orange.
Tithonic yellow, &c., &c.,

the expressions pointing out the region of the spectrum under consideration, but drawing a strong and perfect distinction between the agent involved and light. In the same way other derivates will arise, such as tithonic white or black.

331. When a beam of light is dispersed by the action of a prism, and the resulting spectrum examined by physical tests, it is easy to recognise that the quality of imparting a sensation of difference of colour is not the only token of intrinsic difference in the character of rays of different refrangibilities. Writers on the mechanical theory of optics have assumed, that the constitutional distinction between the various colour giving rays is the different lengths of waves which they represent. To a given index of refrangibility there belongs a particular length of wave, and a particular tint of colour. But there are facts connected with the history of light which seem to prove that beyond this there are peculiarities which are far more profound.

332. Several years ago, Sir D. Brewster showed that, by resorting to absorbent media, rays of any colour could be insulated in every part of the spectrum; that red light existed in the violet spaces, and blue light in the red. These results, being substantiated, would appear to afford a very formidable argument against the dependance of colour on wave length.

333. I do not propose to enter here on any speculative considerations respecting the physical causes which enable light to impart to an organ of vision the phenomena of tints, but to show that the idea of coloration must be admitted for the chemical rays.

334. As, in the foregoing chapter, we gathered our final views respecting absorption from considerations originally drawn from one case; so, in this, let us examine the phenomena exhibited in one or two cases, and then generalize from them. We have already seen (Ap., 597), that when a ray has impinged on a sensitive surface, as on a Daguerreotype plate, and been reflected by it, it has lost, to a great extent, the power of again producing the same effect, or is detithonized.

335. The remarkable process discovered by Sir John Herschel, and called by him chrysotype, enables us to verify in an easy and very satisfactory way the truth of this remark.

336. Case of the Chrysotype.—The sensitive material employed is the ammonio-citrate of iron; *a solution*, which, when viewed through small thicknesses, is of a yellow colour. From its being a solution, it is peculiarly fitted for these experiments.

337. When a piece of paper, washed over with this yellow solution, is exposed to the sun behind a trough containing the same solution, the paper is found to change very slowly, showing that the liquid in the trough is absorbing the active rays.

338. *Prismatic Analysis of the Chrysotype.*—(*a.*) I projected a motionless spectrum on chrysotype paper, and speedily obtained an impression of a pale brown colour, which, when brought out by neutral chloride of gold, was found to extend from α to β, *fig.* 126, x.

(*b.*) Having passed a beam from the heliostat, through a trough with parallel sides,

containing a solution of ammonio-citrate of iron, of such strength, and in a stratum of such thickness, as to appear of a bright yellow colour, I dispersed it by the prism, and received the spectrum, as before, on chrysotype paper. For a long time the paper remained unchanged, but after an hour's exposure I was able to bring out a very faint mark, the position of which was δ, ε, *fig.* 126, *x*.

339. The inference which plainly arises from these experiments is, that the active chrysotype rays are absorbed by the ammonio-citrate of iron; or, in other words, that this substance is sensitive, because it absorbs a peculiar class of rays. No change can take place in chrysotype paper by rays that have passed through ammonio-citrate of iron, because they have been absorbed, and are already expended in effecting the required decomposition.

340. The same conclusion was arrived at by experimenting in the following way: I prepared a sensitive plate by exposure to iodine and bromine successively, which gives, as is well known, a very changeable surface. This plate may be called, for the sake of distinction, a test-plate.

341. On this test-plate I received a spectrum formed from a beam which had passed through the trough containing ammonio-citrate of iron. After a suitable exposure, I found a stain reaching from γ to a, *fig.* 126, *y*. But, as is shown in *fig.* 126, *x*, the rays which affect the ammonio-citrate of iron reach from a to β. Consequently, we perceive that those which affect the test-plate are complementary to those which affect the chrysotype. We draw, therefore, these farther conclusions:

1st. *That the rays which escape absorption by the ammonio-citrate of iron are precisely those which do not affect it chemically.*

2d. *That the rays which are absorbed by the ammonio-citrate of iron are the rays which produce chemical changes in it.*

342. Let us take a second case, selecting for consideration the bichromate of potash.

Case of the Bichromate of Potash.—As is well known, a piece of paper dipped in a solution of this salt speedily turns brown on exposure to the sun's rays; but if there be placed before it a trough containing a solution of the salt, then the change goes on very slowly.

343. *Prismatic Analysis of this Case.*—On projecting a motionless prismatic spectrum on this paper, an impression was obtained in a quarter of an hour, which extended from a to β, *fig.* 127, *x*.

344. A trough with parallel faces, filled with a solution of the salt, was next interposed in the beam, and the resulting spectrum received on a bromoiodized test-plate. It extended from γ to a, *fig.* 127, *y*.

345. In reference to the test-plate used in these cases, its applicability depends on a fact pointed out by Sir J. Herschel (*Phil. Trans.*, 1840, p. 38), that bromide of silver is equally affected by all the rays of the spectrum. In using it as here described, the experimenter must assure himself that sufficient of the bromine has been employed to give sensitiveness to the *extreme* rays of the spectrum; it should produce such a tithonograph as that given in *fig.* 128, the red region being fully brought out.

346. On examining *figs.* 126, *x*, 126, *y*, we perceive that they prove for the bichro-

mate of potash what has been already proved for the chrysotype preparation—that the active rays are absorbed, and that the inactive rays escape.

347. Without dwelling longer on the detail of farther instances, it appears that the general laws under which these phenomena take place are as follows:

1st. When a ray impinges on a sensitive surface, or passes through a changeable medium, with the chemical effect that takes place, the constitution of the ray is correspondingly disturbed. A change in the composition of the medium involves a change in the ray.

2d. Rays which thus disappear by absorption are occupied in disturbing the constitution of the ponderable medium.

3d. Rays which are inactive, or which are not involved in the chemical change going on, escape from the medium by being transmitted or reflected.

348. The definite views which we thus gather respecting the absorption of the different constituents of the solar rays, and the production of chemical changes, lead us by very simple steps to regard one as the cause and the other as the effect. In cases like these, the safest way to true conclusions is, to be guided by analogies. It is true, that the properties of the different agents in the solar beams are sufficiently distinct, but, as radiant principles, they have certain qualities in common. The heat of a sunbeam converged by a lens on red oxide of lead is absorbed by that substance; oxygen gas is given off, and a lemon-coloured protoxide remains behind; no farther absorption of heat now takes place, and no farther chemical changes ensue. Heat, therefore, as well as light, or the tithonic or phosphoric rays, in producing its effects, undergoes absorption.

349. In thus making the phenomenon of absorption the fundamental fact of our theories on the chemical action of the sunbeam, and in giving a distinct prominence to it, a great deal of precision will be brought into our theoretical discussions. Almost every experimenter has, to a certain extent, recognised the truth of these views in a general way, though without clearly setting forth the exact conditions under which absorption takes place. In 1841 (Ap., Ch. XII.) I published some experiments to show how completely all the chemical changes effected by light were under the control of absorption, and that the sensitiveness of any given changeable compound could be altered by altering its optical constitution.

350. So, in the case of radiant heat, a piece of polished silver exposed to the focus of the most powerful burning mirror never melts, not because it is an infusible body, but because such an optical constitution has been given to it, that it reflects the heat which impinges on it. If the polish be taken off, and the surface slightly roughened, it melts in an instant, because it can absorb the rays. So, too, different coloured pieces of cloth, exposed to the sunshine upon snow, will sink to different depths, because the quality of coloration which they possess enables them to absorb the heat more or less rapidly, and the calorific effect is determined by the optical constitution.

351. Apparently nothing can be of a more irregular character than the photographic impressions and points of maxima left by the solar spectrum on various surfaces. With trivial causes the position and dimensions of those impressions change, but when we

come to consider their mode of origin, as thus connected with absorptive influence, nothing is more plain or easy to understand. We must regard them as phenomena of exactly an equivalent character to those of the different appearances exhibited by the luminous spectrum when it is received on variously-coloured paper. For, in respect of the tithonic rays, surfaces that have the same tint to our eyes act like surfaces of different colours. A solar spectrum received on a surface of lampblack is scarcely visible. On a piece of red paper the red and orange rays are copiously reflected, the others more or less absorbed. On a yellow paper the yellow and orange are brilliantly given, but the blues almost disappear; on a blue surface the more refrangible rays are brightest, the yellows have faded away. So, when sensitive surfaces are exposed to the spectrum, they give us an expression of their particular action. Bromide of silver absorbs more uniformly than any other body that we know rays of every refrangibility. In respect, therefore, of the tithonic rays, it acts as lampblack does to the luminous, and might be regarded as a black body. Iodide of silver absorbs the blue, and reflects the red, the orange, the yellow, and part of the green. To eyes, therefore, which could perceive those invisible rays which it reflects, it would be seen as though acting as a ruddy-coloured body, and giving forth rays like those which nitrous acid gas transmits.

352. When, therefore, we find on a sensitive surface which has been exposed to the spectrum a given stain, we infer that rays corresponding in refrangibility to the place of the stain have been absorbed, and the rest reflected or transmitted. And if this be true, our views will be greatly facilitated if we resort to some simple method of nomenclature, which shall be descriptive of the facts observed. It is for this reason that we have proposed to recognise the phenomenon of coloration for these dark rays, and speak of red tithonic, yellow tithonic, or blue tithonic rays, as pointing out in a general manner the place in the spectrum of the ray, the properties of which we are discussing. Extending these ideas to the physical characters of ponderable bodies, we would assert, with MELLONI, that they have an invisible coloration of their own; that bromide of silver is tithonic black, though as respects light it is white; that iodide of silver is tithonic red, though as respects light it is of a lemon yellow.

353. Aided by these definite views of absorptive action, and the concomitant phenomena of coloration, it becomes interesting to examine whether these principles are applicable to cases in which chemical changes are brought about by the action of LIGHT. It is with a view of showing that all these things hold in the decomposition of carbonic acid by leaves under the light of the sun, that we have entered on this minute discussion.

354. The law under which the discharge of vegetable colours in the solar spectrum takes place has not escaped the penetration of Sir J. HERSCHEL, who has furnished us with so much that is new in this department of science. "The rays effective in destroying a given tint are in a great many cases those whose union produces a colour complementary to the tint destroyed, or, at least, one belonging to that class of colours to which such complementary tint may be referred."—(*Phil. Trans.*, 1842, p. 189.)

355. Now this is nothing more than an expression of a particular case of absorptive decomposition, in which light is the agent, and vegetable matter the substance involved.

The reason that a yellow substance is bleached by blue rays, *is because it absorbs those rays*, for the very same reason, therefore, that it looks yellow. A purple vegetable body is bleached by the yellow and green rays, *and because it absorbs those rays it looks purple.*

356. As respects light, the phenomena of coloration are obvious to our organs of vision; as respects the dark beams of heat, of chemical action, and of phosphorescence, they must be hypothetical or ideal; but, in the same manner that Melloni has found the admission of them for the calorific rays of such admirable advantage, so, in this department of science, similar and palpable advantages arise. Who could for a moment doubt that light and the tithonic rays were agents totally distinct, so soon as he came to understand that to one of them iodide of silver is yellow, to the other orange; to one chloride of silver is white, to the other red; to one bromide of silver is white, to the other it is black?

357. These principles undergo a severe test when we examine the phenomena that arise when sensitive surfaces of different degrees of thinness are used. As is well known, if a silver plate be exposed to the vapours of iodine, it passes through several orders of colour, red, yellow, blue—red, yellow, blue, &c., &c. In each one of these series, as I formerly showed, the yellows are chemically the most sensitive (Ap., 622, &c.).

358. If, therefore, we successively expose to the solar spectrum thin plates of iodide of silver of the above-named tints, we might infer that the resulting impression should change its position with the colour of the plate; that, if that colour was yellow, the rays corresponding to the blue should be absorbed, and the spectrum impression be found among the more refrangible rays; that, if the colour was blue, a tint arising from absorption of the yellow, a spectrum stain should be found corresponding to the position of the yellow rays, and not to the blue, these last undergoing reflexion; and, finally, as the colour of the plate changed, so should the photographic spectrum shift its position.

359. But, on making the experiment, I found that this result does not arise; it is immaterial what the colour of a Daguerreotype plate may be, the spectrum leaves upon it an impression in an invariable position. In the absence of diffused light, this impression is entirely among the more refrangible rays.

360. But as the colour of the plate changes, although the photographic impression remains invariable in position, it undergoes variations in intensity. It exhibits the deepest stain when the plate is yellow, is more faint when the plate is red, and becomes hardly perceptible when the plate is of a grayish metallic aspect. Time, of course, enters as an element into these results; a gray colour will receive as deep an impression as a yellow, if the period of exposure be inordinately prolonged.

361. From this we gather, that on exposing films of iodide of silver of different thicknesses, and therefore of different colours, to the spectrum, the resulting impression does not shift its place, but, remaining fixed in position, undergoes variations in sensitiveness—variations which are exhibited by differences in the depth of the stains. And this result is a striking instance of the doctrine of ideal coloration. Upon the principles of that doctrine, it meets with a beautiful explanation.

362. The iodide of silver is a body which copiously absorbs blue tithonic rays, a quality arising from its chemical relations and constitution. It is unaffected by the pure yellow and red tithonic rays, when they operate alone, and diffused light is excluded. Consequently, it can give no indications dependant on the presence or absence of them. The sunlight to it is monochromatic, or nearly so, for it is decomposed by blue, indigo, and violet rays only. The phenomena, therefore, which it will exhibit when in thin plates, are such as would be exhibited by thin transparent plates of air, or water, or glass, on which a monochromatic light is falling. If monochromatic light falls upon plates of glass of variable thickness, the reflected beam simply changes in intensity, and, if the language of science permitted us to describe bodies in their optical relations as being more sensitive to light when they failed to reflect it to our eye, and less sensitive the more copious the reflexion, we can understand that the plate of glass would pass through all orders of sensitiveness as its thickness varied. At one time it would reflect all the incident beam, and as it increased in thickness the ray would diminish in intensity, and finally disappear, and, with a still farther increase, the brilliancy of the beam would again be reassumed, and so on through successive periods.

363. *Now this is absolutely the same phenomenon as that exhibited by iodide of silver in films of variable thickness when exposed to the spectrum. It depend on the ideal coloration of the iodide.*

364. Let us examine, in the next place, what should be the event when bromide of silver is used instead of iodide.

In order to enable us to predict the result, we have simply to consider what would occur if a film of lampblack, or of any other perfectly absorbent body, could be obtained, of suitable thinness, and exposed to the luminous spectrum. For, to use a somewhat objectionable, but perhaps emphatic expression, bromide of silver is the lampblack of the tithonic rays. It is obvious that a perfectly absorbent film would uniformly appear black, no matter what its thickness might be. The tints of thin plates arising in interferences among reflected or transmitted rays must, in this instance, be absent. A film of bromide of silver must, therefore, have a uniform sensitiveness—a sensitiveness which is independent of its thickness.

365. Having thus explained the laws of absorption and the doctrine of ideal coloration in this and the preceding chapter, it remains only to add a few words on the mode of action of the tithonic rays.

366. There is no reason to believe that oxygen, hydrogen, or nitrogen gases, in masses of ordinary magnitude, exert any perceptible absorptive effect on light, heat, or the tithonic rays. These bodies, therefore, and all others having the same relation, can exert no action on each other, even though they are under the influence of the most intense radiation.

367. A mixture of oxygen and hydrogen gases, exposed to a brilliant light, can never produce water, because neither of its constituents has the power of absorbing the incident rays.

368. But a mixture of chlorine and hydrogen gas explodes in an instant under the influence of light, because the chlorine can exert a powerful absorbent action.

369. From these theoretical considerations it would appear that, in the case of chlorine and hydrogen, the latter gas is wholly passive; the chlorine, being acted upon, absorbing the chemical rays, is thrown into such a condition that union takes place with the hydrogen.

370. A remarkable consequence follows from these views. If the reason that oxygen and hydrogen cannot form water under the influence of the sunlight be due to the circumstance that neither of those gases can absorb tithonic rays, but are perfectly transparent and colourless, and the reason that chlorine and hydrogen at once form muriatic acid, be due to the absorbent capacity of the chlorine, it results that when a mixture of these latter gases intercepts a ray, the absorbent action upon that ray should not be greater than that of the chlorine alone, and even not more than one half, because of the diluted state in which the chlorine is presented. But this is the same conclusion to which we have previously arrived by direct experiment (321).

371. The views which have been given in this chapter serve to show that chemical action is the uniform result of absorption; but the converse of the proposition does not hold good; absorption is not necessarily attended by chemical action. Nevertheless, it is attended with a certain effect. Even in the case of an elementary, and therefore unchangeable substance like chlorine, a disposition or capacity for union is communicated. Chlorine that has been exposed to the sun unites with hydrogen more readily than chlorine which has been made and kept in the dark (Ap., Ch. XVIII.),

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CHAPTER XI.

ON THE MODE OF ACTION OF LIGHT IN DIRECTING THE DIGESTION OF PLANTS.

Contents: *Connexion between Absorption and Chemical Action.—Radiant Matter is absorbed in producing different Effects.—Reappearance of the Force expended.—Laws of Preliminary Absorption and Definite Action observed by Plants.—Increased Rapidity of Vegetation implies increased Brilliancy of the Incident Light.—The Sun probably a Periodic Star.—Description of the Mode of Action of Light and Radiant Heat on Leaves.*

372. We are now ready to take up the consideration of the question proposed in Chapter II.: In what manner does light act in directing the digestive function of plants?

373. From various phenomena exhibited by radiant heat, M. Melloni has developed the doctrine of invisible calorific coloration, those of latent heat and absorption having been established many years ago. In the same manner, from phenomena connected with the tithonic rays, we have developed for them analogous doctrines in the preceding chapters. As respects light, the views which, for the other imponderables, are only ideal or imaginary, for it become certain; because our organs of vision inform us

at once. A ray of light, dispersed by the action of a prism, presents to us a spectrum in which we see plainly intrinsic differences of the various parts. Exposed to this spectrum, or to these coloured spaces, vegetable juices of different tints undergo modifications; some are changed by the blue, some by the yellow, some by the red ray. If we examine the conditions under which these things take place, as we have done for heat and for tithonicity, we find similar evidences of absorption. Thus, chlorophyl, which gives a green colour to leaves, undergoes a change by light, and becomes white; and when, by prismatic analysis, we inquire what rays are active in producing this effect, we find that they are those which have been absorbed.

374. For plants, therefore, coloration is intimately connected with chemical action. There is a chemical reason why leaves are green and flowers are never black, but unfold a painted corolla; and, guided by these principles, we can see that all those various active principles which are stored up, all the different juices which circulate, are connected with, and have borne a certain relation to the coloured portions. In a rose, the leaves of which are green, and petals red, and interior organs of reproduction of a gaudy yellow, these tints do not result from a wanton play of Nature, but are rather evidences of premeditation and forethought. As anatomists have been enabled to deduce from their studies a testimony for the existence of a Universal Designer, so, too, might chemists, from reasons on the colours of a wild flower, show that those colours are the *means* of accomplishing certain *ends*.

375. These things hold, whatever doctrines we assume respecting the nature of light; whether, with Newton, we regard it as consisting of emanations of particles which are exceedingly small, or, in the more refined views of modern philosophers, as undulations of an elastic medium. The waves of sound which pass through our atmosphere in like manner produce striking and permanent results, and even are often connected with those higher philosophical events which not only belong to material things, but also to the world of intellectuality. There are strains of music which have been listened to in youth, and have communicated a permanent impression to the brain, which, in after life, spontaneously present themselves to the memory. It is true that these things originate in intellectual operations, but it is equally true that the channels of communication through which they have passed from mind to mind belong to the inorganic world. It is atmospheric pulsations which are thus registered in the brain. In that wonderful organ they are stored up, and amid the hourly change of every part of the living system, the constant introduction of new particles, the passing away of those which are dying or effete, these aerial pictures are permanently preserved. In an instant, and spontaneously, there flashes across the memory a recollection of events which transpired half a century before, and which have been buried in oblivion. So, also, with the undulations of light. The effect of these in no case passes away, but leaves its permanent impression on material things, and these impressions, though for many ages they may lie dormant, reappear again at their proper time, and produce their proper effect. Ten thousand centuries ago the sunbeams fell on the leaves of trees, and decomposed carbonic acid just as they do now; and the woody matter they produced was buried in the earth. In natural affairs no such thing as a system of expe-

dients is known, but all results from the operation of far-reaching and immutable laws. In those remote times events were taking place, the application of which referred not to things then existing, but to an hereafter. The earth, and the sea, and the air were enlivened and invigorated by the sunbeams as they are now, but while present purposes were subserved, the future also was not forgotten. In the twilight that then existed, as it does now, the wolf followed his flying prey, and by the light of the same stars the royal tiger pursued his midnight maraudings. Nor is there anything opposed to the order of Nature in those things. Intellectual development can only take place when a thousand natural conditions conspire. Reason and analogy would equally lead us to suppose that, in a majority of those globes which are scattered through the regions of space, those conditions are not attained; that they are, as this earth was for countless ages, a dungeon of pestiferous exhalations, and a den of wild beasts. And yet the formative forces of Nature are at work; the plastic fingers of light, each colour producing its proper effect, are arranging, and decomposing, and modelling, and the work which thus goes on in silence attains perfection in the lapse of ages. In those planets, as in this, the atmosphere and the sea are finally brought to a proper constitution, and wild animals make their appearance, and then intellectual beings. For these, whatever is wanted has been provided. And results which for so long a time have lain dormant now come into use, just as those forces which in primeval ages were expended by light in the reduction of carbon by leaves; carbon which has been buried in the earth, in our times reappears again, and is, perhaps, occupied in driving the steam-ship that carries these pages over the waves of the Atlantic Ocean.

376. Two prominent facts must always be present to the mind of a natural philosopher—the indestructibility of matter, and the indestructibility of forces. Disappearances of the one or of the other are only fictitious deceptions. Throughout the universe the quantity of matter and the quantity of force remains forever unchanged. Material atoms migrate from one condition to another, now putting on the form of a solid, now of a liquid, now of a gas; so also with forces, which are occupied in producing sometimes one, and sometimes another effect; but the quality of the former and the value of the latter remains, under all circumstances, unchanged. And when such considerations as those which are before us show that rays of light of different colours have certain offices to discharge, and certain chemical effects to produce, affinities of given intensities to overcome, molecules to group in determinate positions, these are things which can only be done by the expenditure of a certain force; but, though that force be expended, it is not destroyed; it is ready to reappear from its condition of transmutation. In the same way that, all over the American Continent, at a certain period after the fall of the leaf the Indian summer sets in, and in the cold weather of winter restores the warm days of July, the heat which is evolved from decaying leaves in the forests being derived from the sunrays of the preceding summer, so also every ray which has been expended, no matter of what colour it may be, or what has been the chemical or physical result with which it has been connected, or what series of transmutations it has passed through, is ready to be restored in its pristine energy, and give rise to its equivalent mechanical effect. And hence we can see that although, so far

as the whole universe is concerned, the amount of force never varies, there are periodical variations in its distribution. The solar rays which year after year are impinging on our earth, are so much taken from him, and so much given to us; and, judging from these things, we should infer that the amount of force exhibited on the surface of our globe should steadily be on the increase. Are not these also the ideas which we conceive from geological investigations? And is it not this which, from a silent and tenantless waste, has made that surface the abode of myriads of living things? that produces all over it locomotion and activity, and that makes all the difference between what our earth was at the beginning of the secondary epoch, and what she is now?

377. It was an observation of the older botanists, that the green parts of plants only possess the quality of reducing carbon from the air. In other portions, and under certain circumstances, a reverse action takes place, and carbon, probably under the form of sugar, is oxydized. This never takes place in the presence of chlorophyl. The analogies which we have traced between the mode of action of light, heat, and the tithonic rays, would serve to indicate, that in this remarkable decomposition the luminous rays undergo the same changes, and, indeed, act in a similar way to radiant heat when it gives rise to decompositions, or the tithonic rays when they produce surface alterations. From these analogies, we should judge that the active rays in this instance undergo a true absorption, which is, perhaps, divided into two periods, as we have seen is the case when chlorine and hydrogen unite (317), or iodide of silver is decomposed. That the preliminary absorption is observed, numerous facts seem to indicate. When leaves, immersed in water containing carbonic acid in solution, are set in the sunshine, they do not all at once commence evolving gas, but a certain period of time elapses before the bubbles pass off with any rapidity, and that period once over, they seem to follow the fluctuation of light with considerable accuracy. A cloud passing before the sun restrains the speed of reduction, and an increased brilliancy of light is followed by an increasing rate of decomposition. From the mode in which these experiments are necessarily made, there is not a perfectly clear proof of the preliminary absorption, for it is not impossible that the hesitation in evolving gas which is observed comes from disturbing causes. It may require a certain time for the carbonated water to find its way through the tissues of the leaf, and to reach the seat of action. It may also require a similar period of time for the evolved gas to percolate out. But still, allowing for these disturbing actions as much time as might be reasonably supposed sufficient, there can be little doubt that, when leaves are brought out of the dark and set in the sunshine, a certain period elapses before vigorous action sets in—a period which seems to correspond to that of the preliminary absorption observed in other cases.

378. If any doubt should remain on that point, there can be none on the circumstance that light observes the law of definite action. Not only is this apparent from the phenomenon taking place with a rapidity corresponding to great increases and diminutions of light, but also that minor differences are rigidly observed. Even in the spectrum, we have seen that the rate of decomposition follows very closely the order of illuminating power, and here we have rays of various refrangibility and of different colours in action. If, under such circumstances, where variations of colour intervene,

the course of the phenomenon seems to be directed by intrinsic brilliancy, there can be very little doubt that, for the same colour, or for white light, the law is rigidly observed, a brighter illumination or a greater quantity of light producing an increased effect, a lesser illumination or a feebler light producing a diminished effect.

379. It seems to be substantiated by geological facts, that in former ages the rapidity of vegetable growth was far greater than it is now; that certain plants, which with us attain only to an insignificant size, in those times reached a very great magnitude. So ferns, which in our latitudes are now of an insignificant growth, in the same places were formerly evolved into trees. The general character of vegetation, also, in given latitudes, points to a former period of greater luxuriance—a period during which tropical trees could grow in the temperate zone. Geologists have already concluded, from these and a variety of other observations, that the surface temperature has undergone a diminution.

380. But there are many conditions which have to be fulfilled before so marked a difference could have taken place. The rate of vegetable growth depends on many things: on the amount of aqueous vapour and carbonic acid in the air, on the mean temperature of the surface, on the brilliancy of the incident light, &c. There are experiments which seem to show that the constitution of the atmosphere is by no means so favourable as it might be; a greater quantity of carbonic acid in it is attended by a greater rapidity of growth, and since those times of which we are speaking, great changes have taken place in this respect. Of the carbonic acid of those eras much is now removed, and shut up in the earth in those great deposites of anthracite and bituminous coal which occur on an extensive scale in so many parts of the world. Large quantities, also, unquestionably derived from the same source, now form an integral constituent of great coral reefs and limestone rocks, some forming mountain ranges, and some in the sea. It can admit of little doubt that, since those times, the total quantity of organized carbon existing on the earth's surface, and constituting the parts of plants and animals conjointly, has been on the increase—an increase attended by a diminution of the quantity unorganized in the atmosphere. M. Dumas has well observed, that the original atmosphere has become divided into three parts: one which still, in a modified form, envelops the earth on all sides, constituting its present atmosphere; a second, represented by the aggregate of vegetables and animals now existing on the earth's surface—for plants and animals are nothing but condensed air; a third, enveloped in a fossil state in the bowels of the earth. And this tri-partition has been mainly effected by the agency of the sun. To restore things to their primordial condition, all these must be mingled together in the gaseous form, and the forces that have been derived from the solar rays restored back to that luminary again.

381. But an excess or a diminution of carbonic acid in the air, provided the variation is within certain limits, would not exert an exclusive control over the production of vegetable organized molecules. Variations of temperature, as common observation shows, exert a very great effect; the periodicity in the seasons and processes of horticulture are sufficient to prove this. So well, indeed, has this been understood, that geologists, from considerations on the decline of vegetable growth, have drawn the doc-

trine of diminution of terrestrial temperature; a doctrine which is enforced by arguments furnished as well by the inorganic world.

382. But radiant heat is not the primitive force which organizes the carbon atoms, and groups them into their various forms; it acts a subsidiary part, the decomposition and subsequent arrangement being directed by LIGHT. Whatever facts, therefore, exist, which prove an increased activity in vegetable growth in the early times, prove also an increased brilliancy in the light. The occurrence of an excess of carbonic acid in the air, or of a higher temperature, is not enough. To the light, which is the vital agent, a greater activity must be assigned. It is thus we perceive that changes in the interior temperature of the globe can have had only an indirect connexion with what was thus going on on its surface. And if there have been periodic vicissitudes, if plants have once grown with excessive luxuriance, and in short spaces of time withdrawn large quantities of carbon from the air, this is a result which is connected not so much with internal or external temperature as with periodic variations in the brilliancy of light.

383. Do not, therefore, these things seem to indicate that our sun is one of those periodic stars, the light of which undergoes secular changes; that for a series of years or of centuries it increases in brilliancy, and then fades away; and that, as these periods pass over, corresponding mutations in its intensity of radiation are observed? That, affected by this, the rate of vegetable growth, the character of animal life, the constitution of the atmosphere is simultaneously changed in all the attendant planets? It is of no consequence to say that great and almost universal mutations, such as those we are here describing, are not consonant to the ways of Nature; or that, in the periods of human history, traces of such operations have never been witnessed. No observation in philosophy is more true, than that "changes which are rare in time become frequent in eternity."

384. But among the stars these periodic variations do take place. As was discovered by Sir J. HERSCHEL, *a Orionis*, if examined, is seen to increase in brilliancy for several days, and then to diminish. In the same way, *a Cassiopeiæ* has its period embraced in 225 days. And many other instances are known. It signifies nothing that these periods are short. In the constitution of the universe, no value is attached to time. With men, whose period of action is embraced in a few years, the different events of life are circumscribed by measured spaces—there is a limit beyond which human exertion cannot go; and to adjust time and action to each other, and to measure the one by the other, is our common duty. But in the administration of the universe the case is different; in eternity there are no limits of duration, and time can be expended without detriment or loss. In the pulsation of a wave of light, a part of the millionth of a second is enough, and it is given. In the revolution of one system of stars round another, millions of centuries are required, and they are consumed. And so, in the case that we are considering, the glowing and fading away of one star may be accomplished in a few days, but inconceivably great periods of time may be wanted for the same events to transpire in another. Even philosophers are too prone to believe, that by the short spaces of human life, or the history of nations, they can mark out pe-

riods in eternity; but, whether we consider the scale of space, or of time, on which the universe is constructed, we can see that our minds are so constituted as to be equally unable to appreciate either extremity; that we can attach no just idea to what is either infinitely great or infinitely small; and that therefore our views do not always justly apply in natural events. Of one thing we may rest assured, that no matter how great the periods that may be required for the phenomena of the universe to transpire, there has been, and there will be time enough for their endless repetition.

385. From considering what takes place when a green leaf is enlightened by the sun, we are thus allured to pass on step by step to reflections on the history of the solar system, and to changes which have happened to the earth. In the same way that the stem of an exogenous tree is a lasting record and memorial of the returning summers it has witnessed, each ring that we see pointing out the growth of one season, and being, so to speak, an index of the amount of light which has been at play; or, when casting our eyes over the climates of the earth, we observe in the tropics a rank vegetation, and trees and flowers flourishing all the year round; or, coming to the temperate zones, we find a hardier growth, and the soil only yielding its fruits to human industry and skill; or, passing towards the poles, the stunted plants, and lichens, and mosses, and great plains covered with perpetual snow, and even in these inclement regions all vital operations under the control of astronomical causes. Do not all these serve to set forth the entire control which the solar rays have over these phenomena, and teach us that the same kind of reasoning which applies to things taking place in our time, applies also to things which have preceded it; that if the section of an exogenous stem, or the climate-distribution of plants, point out a present connexion and present relations with the sun, so do those fossils which are dug out of the ground point to similar relations in former times; their magnitude and luxuriance indicate a more brilliant ray. From the beginning of things no natural law has ever changed; results are obtained in these times by the same operations or mechanism by which they were obtained of old. As with us, so then, when the sunbeam falls on a leaf the yellow ray is absorbed, and if carbonic acid is present, it undergoes decomposition, green matter is rapidly formed, a mixture of oxygen and nitrogen gases is emitted, and carbon, oxygen, hydrogen, and nitrogen are fixed in the plant. The seat of this action is now, as it was then, that face of the leaf which is exposed to the sky; and the nutritious juice thus formed turning over to the under face of the leaf, is there concentrated by the evaporatory action of the stomata. From these chemical changes mechanical forces arise, and the nutritious sap is impelled downward, or brought into relation with all parts of the plant. From it sugar, and gum, and starch, and woody fibre, albumen, fibrine, &c., are formed, and these are lodged in various parts, or stored up for the farther purposes of the economy.

386. I regard, therefore, the sunlight, when acting upon plants, as operating exactly in the same way as the chemical rays or radiant heat, when they produce their specific phenomena. That, first of all, a certain absorption takes place, which seems to be unattended with any direct effect, and of the nature of which we have only an indistinct idea; that when this is over, the rays continuing to act, and the tissue of the leaves

being filled with water holding carbonic acid in solution, part of which acid has been derived from the air, and part brought through the spongioles from the soil, decomposition takes place—a decomposition accomplished under the law of the definite action of light; that if the rays increase in brilliancy, the chemical result goes on with more rapidity, and if there be a diminution, the chemical result correspondingly declines; that this action goes forward at a maximum under the influence of the yellow ray, the orange and the green coming next in rank, and the others following in the order of their illuminating power; that from this circumstance, the extreme violet and extreme red seem to possess little activity, and the tithonic rays appear to be in no manner engaged, or engaged only in an indirect way, after the same manner as radiant heat. As fast as carbonic acid, dissolved in the vegetable juices, is disposed of, new quantities are taken up, some little coming with the ascending sap from the ground, but the great part being supplied from the air; for, through the air, by diffusion, gases pass with great rapidity, and percolate through their films of water (Ap., 80). By aerial currents, by the movement of the leaf an extensive and continually renewed contact with new portions of air is established; from this the carbonic acid is taken, which is dissolved in the watery juices circulating. Brought under the luminous influence, it undergoes decomposition, its carbon and a portion of its oxygen being appropriated (Ap., 818), and a volume of nitrogen equal to the volume of oxygen thus appropriated, evolved along with the remaining oxygen. There is, therefore, a removal of water and carbonic acid from the air; a fixation of carbon, hydrogen, oxygen, and nitrogen in the plant, and an evolution to the air of nitrogen and oxygen. From the constant appearance of the former of these bodies, we are led to suspect that the light acts primarily on some azotized body, the destruction or eremacausis of which is essential to the total action (Ap., 824); while all this is going on, chlorophyl is abundantly formed, and so long as the process is accomplished, the leaves retain their green colour.

387. On several occasions it has been said, that the phenomenon here described answers to a true digestive, and not to a respiratory process; the older chemists and botanists confounded it with the latter function; but it is obvious that it does not answer to respiration, either in mode of operation or in result. Respiration is an oxydizing process, the object of which is to maintain the animal machine at a fixed thermometric point—a result which implies direct combustion or burning; an animal, rigorously speaking, burns carbon like a locomotive engine. But in this action, on the contrary, carbon is reduced from carbonic acid, and there should be a descent of temperature instead of an elevation, a large amount of heat being absorbed—heat which is furnished, under natural circumstances, by the sun, along with his light. The continued supply of heat in this way prevents us from discovering that reduction of temperature which should befall the leaf; and, besides this, it is exposing to the open atmosphere its broad surface, and any thermometric disturbance is at once compensated by external agencies. It is possible, indeed, that the decomposing process could not go on, save under the conjoint presence of heat and light, though the specific function which each of these agents discharge may be different. The action of leaves in the sunshine bears, therefore, no sort of analogy, either in manner or in result, to the respiratory processes

of animals, no matter whether the mechanism be the lungs of a mammalian or the branchial organs of fishes. Every variety of breathing apparatus has for its object the evolution of heat by the oxidation of carbon, or of carbon and hydrogen conjointly; but the object of the agency of leaves upon the air is to obtain from it carbon, or carbon and hydrogen. At certain periods of their history, plants themselves become machines of combustion, when the process of fertilization requires that for a time, in a given place, and for a specific object, there should be an elevation of temperature. Resort is then had to those same processes which obtain in animal systems; and sugar, or what comes to the same thing, honey, is burned.

CHAPTER XII.

ON THE NERVOUS AGENT OF PLANTS.

CONTENTS: *Subdivisions of Nervous Mechanism in Animals.—Excessive Rapidity of Motion arising in these Nervous Actions.—Plants constructed on a Surface-type.—Oxidating Processes replaced in them by the Application of Radiant Heat.—Difference of Action on the Upper and Under Face of the Leaf.—Light applied to one, and Heat to the other Face.—Specific Effects produced by the different-coloured Rays.—Effects of these Radiant Principles on the Lower Tribes of Animals.—Centralization of Apparatus for different Functions.—Analogies between Nervous Action in Animals and Imponderable Agency in Plants.—Vegetables are the Representatives of the Resultant Action of the Ethereal Agents on Ponderable Matter.—Conclusion.*

388. IT was the beautiful discoveries of Sir C. BELL which first forcibly drew the attention of physiologists to the fact that different portions of the nervous system are devoted to different functions; that in the spinal axis there is one column devoted to sensation, and another to motion. This division of offices is doubtless carried to a far greater extent than we have at present any means of proving. Analogy would lead us to suppose that every function is represented by its own appropriate mechanism. It is not alone in the great and more striking characteristic divisions of action that these divisions of machinery are observed; the cerebrum, the cerebellum, or the sympathetic system, being each devoted to its specific end, they doubtless, also, exist on a far more minute scale, in connexion with more trivial purposes.

389. If, thus, the intellectual processes and processes of movement, which are things appertaining to the interior constitution of the animal system, are under the control of a divided agency, a similar plan is resorted to in the case of those functions which put the system in relation and communication with the exterior world. On these, the outward physical agents have to expend their operation. The optic nerve, which gathers on its retinal expansion the images of outward forms, transmits them to the brain. To that cerebral tract to which it goes, the power is given to be affected by luminous

agency; it is immaterial whether that agency consist of undulations of an ethereal medium, or spend itself in producing a chemical change of the retina. The *portio mollis* of the seventh pair, also, exposes itself in the cochlea of the ear, and having the function of audition committed to it, vibrates correspondingly to those oscillatory movements in the atmosphere which constitute sound. So, too, with the olfactory nerve, which, pushing its way through the cribriform plate of the ethmoid bone, expands in a million of ramified branches on the Schniderian membrane, and is ready to be impressed by odours or smells. There is no such thing as a mutual convertibility of the offices of these different machines; no vicarious interchange of action; each one has its own duty to perform, each has to discharge its proper task, and the construction of each is suitably arranged. In human contrivances, the same necessity of result arises; the telescope will not answer for a piano, nor the piano for a telescope.

390. While thus the different senses of sight, of smell, of hearing, of taste, and of touch, and all the different functions which occur in animal frames, are carried on by their own appropriate enginery, and resort had to optic, auditory, olfactory, and respiratory cords, these several contrivances are so arranged that the final result of their operation converges inward, and is at last expended on the same point from which also spring all the various acts of thought and intellectuality. Of that central point which is thus in incessant agitation during the continuance of animal life, perpetually receiving impressions from outward objects or the external world, perpetually, also, reflecting back again the various determinations of the mind, how rapid and how constant must be the movement! We cannot comprehend how it is possible that an ethereal particle, vibrating so as to produce violet light, oscillates backward and forward seven hundred and twenty-seven millions of times in the millionth part of a second; yet this is a fact as well established as any other fact in the domain of science. What, then, shall we say of that central point of reception which is within the brain, which stands ready to execute at once all the synchronous movements impressed upon it by the various colours of light, all the vibrations of simultaneously occurring and harmonious sounds, all the impressions which are brought to it by the apparatus of the senses! During the time of wakefulness, how is it agitated by these various movements! and during the time of sleep, its activity is still expressed by those phantoms which we see in dreams. As, at a telegraphic station, the observer watches the various signals through his telescope, and reports to his government the intelligence which is arriving, so that central vibrating point reports to the MIND the telegraphic despatches that are coming along the different nerves. Nor is it impossible that one material atom, or even a small congeries of atoms, should be able to be thus affected at once in a thousand different ways; a particle of water on the surface of the ocean may be simultaneously affected by millions of waves, which may go forth from it and separate without disturbing the movements of each other; a particle of ether may be acted upon by every possible ray of light that can reach it—it will be affected by the general action of all, and each one will go forth and separate from all the others, undisturbed by, and undisturbing them. But even were it not a single physical point—a physical point in our ordinary idea of that term—even were it the whole brain which is thus agitated and acted on, what but

a point in comparison with the world from which it gathers its intelligence, is the whole mass of the brain! Is it not affected by light which has come from systems of suns at the uttermost ends of the universe? Does it not watch the rotation of double stars of different colours, which occupy enormous periods of time to complete their revolutions, and are situated at almost immeasurable distances?

391 Nervous action in animals is thus carried on by a complicated mechanism, and the nervous agent, though ordinarily spoken of as "one and indivisible," presents itself to our view as the reunion of many forces, or as one force with many modifications. To the immaterial and responsible principle which is within us, it stands in the character of a minister and messenger, to connect it with the outward natural world, to gather impressions and enable it to react on material things. Hence arises that subdivision of functions of which we have been speaking, a subdivision not only affecting external impressions, but also the corresponding individual actions. The nervous and optical mechanism of the eye is so arranged as to have entire charge over the reception of impressions conveyed by the luminiferous ether; the auditory mechanism of the ear is constituted so as to receive undulations of gaseous bodies like atmospheric air; and, correspondingly, if intelligence has to be communicated to a distance, and received by other minds through the agency of a visual organ, the motor nerves of the hand are put in action, the fingers move, and letters appear upon the paper.

392. In steam-engines or other automatic machines, our admiration is chiefly aroused by the regular and consentaneous manner in which their movements are performed. At the proper instant the proper valves are sprung, and the intricate motions go on with regularity. No one can have seen a large cotton or silk factory in which the machinery is driven by one of these engines without being struck with this remark. Perhaps it is weaving an elaborate pattern, and, with a discretion that almost simulates intelligence, is putting in or leaving out the variously-coloured threads. To produce this, a series of contrivances is made to intervene between the point of action and the motive force, and accordingly as the result is different, so must that intervening mechanism vary. In animal systems, the same observation holds good: impressions which come to us from the external world, and movements which result from mental operations, are all transmitted by their proper channels.

393. From the time of Aristotle, it has been observed that the type on which vegetables have been constructed bears a remarkable relation to the type of animals. In the latter all the processes of organic life, such as digestion, respiration, secretion, are carried forward in interior cavities; in the former they are surface actions. Thus, the stomach, the digestive organ, is enclosed in an interior space, but the leaf, which is the analogous organ for plants, is freely exposed, and digestion takes place upon the surface. So, too, with respiration, the lungs of the mammalia are placed within the walls of the chest, and receiving oxygen from the air, transmit it by the arterial blood to the capillary vessels, in which the combustion of carbon is going on; but when in plants the same result is to be attained, oxydation takes place on the petals of the corolla, which is another surface action. In animals the attempt is to centralize everything, to make all the functions subservient to the perfect development of one focal point, to

which all the nerves of sensation go, and from which all volitions and motions arise. In plants, the attempt is to diffuse everything, to have no centre of action, but to execute upon the periphery. It is true that the lower order of animals are constructed on this type of diffusion, and it is interesting to see, when in more advanced tribes the attempt at centralization begins, how rapidly it goes on to perfect development. The appearance of an isolated organ of digestion—a stomach—is the signal of an isolated organ of circulation—a heart—no matter though that heart may be a mere tube, as the dorsal vessel of many insects.

394. If thus, in animal existence, we find the various nervous machines divided off, and the impressions of light, of sound, of taste, committed to separate apparatus, how is it with plants? The rays of the sun are the true nervous principle of plants! And herein we see how closely the type of surface action is observed. On all sides the leaves present their thin lamina to the light, and offer a broad surface to the sky. On these the rays fall, and direct the digestive and other processes. When they are carried on in interior cavities, and the living system assumes a certain mass, an oxydating machine is demanded in order to keep up the temperature of the whole by the burning of carbon, and lungs, or gills, or other suitable contrivances, are resorted to. But the principle on which the vegetable organization is constructed is simple, and except on those particular occasions to which reference has been made (387), an apparatus of combustion is not wanted. For with the rays of light which come from the sun there are also rays of heat, and these impinging on the outside surface, which is the seat of all the vegetable activity, are absorbed, and bring up the temperature to the required point. What need is there for the burning of carbon or of hydrogen, when on all sides radiant heat is pouring in? Nature, with a provident care, has in all ordinary leaves provided abundantly for this elevation of temperature, and when the direct beams do not reach them, warm currents of air that rise from the heated ground are everywhere present.

395. In a chemical point of view, it is the quality of light to produce the organization of molecules, that is, to determine the positions of atoms as respects each other; it is the quality of heat to determine their distances; for the accumulation of caloric in a body produces an expansion, a diminution of it a contraction. Light and heat, therefore, have totally different offices to discharge, and the seats of their respective actions are in most instances distinct. In horizontal leaves it is chiefly on the upper face that light acts; this, looking towards the sky, is ready to receive rays coming from every part; it is on the under face that heat operates, directing in a great measure through the stomata the evaporation of water. An advantage for these latter organs is, therefore, gained. Exposed to the direct solar radiations, they would be under conditions of perpetual disturbance, every cloud which passed over the sun would keep them in a constant agitation, and interfere with their regularity of action. One of the most prominent differences between light and heat is in their mode of propagation through bodies; the force of the former is expended on the point upon which it falls; there is nothing after the manner of conduction or lateral propagation; it is also the same with the tithonic rays (Ap., 608). For this reason, when an image falls on a sensitive plate

in the camera obscura, a photogenic copy is obtained, every line being represented with mathematical accuracy and the utmost sharpness; the radiation, having impinged upon the plate, does not spread itself out laterally or undergo a process of conduction, but only produces its effect on the portions which lie directly in the lines of light. With radiant heat it is very different: a beam of this agent falling upon a limited portion of a metallic plate, slowly affects all the rest, for the resulting rise of temperature is propagated laterally from point to point, and by-and-by the whole mass undergoes an elevation. While light and the tithonic rays, therefore, are not liable to conduction, the reverse holds in the case of heat. Applying these reasons to the instance before us, the rays of light which control the digestive operation could only act in an imperfect way on the under side of a horizontal leaf; they must produce their maximum effect on the surface exposed, and operate elsewhere through imperfect translucency, finding their way through short distances, because the tissues are not perfectly opaque. But the rays of heat, which, with the light, fall on the upper face, are conducted with facility, or transfused by warming the watery juices which are circulating through the organ. Whatever changes, therefore, take place abruptly in the quantity of the incident rays, as when clouds are suddenly crossing the sun, are not abruptly felt upon the stomata, for the leaf, thin as it is, acts as a regulator, and, from the heat which it contains, supplies the momentary defect; the ascending sap also, which is coming through the petiole, is coming at an elevated temperature, for, in the course of its ascent from the roots, it has been warmed in its passage through the stem which is enveloped in its dark-coloured bark—a colour well adapted to receive the effects of the rays of heat. There is, therefore, a reason why the stomata are secreted in the shade, and why the seat of the digestive action lies on the upper face of the leaf.

396. Besides these well-marked differences of action produced by the rays of light and of heat conjointly in the sunbeam, there are specific effects produced by the various coloured rays; thus, the yellow seems to control digestion, and, as Dr. Gardner has proved (*Phil. Mag.*, Jan., 1844), the blue ray motion. Investigations of the different effects produced by the other rays of light, such as the green, the violet, the red, or by those other principles which are invisible; the phosphoric or the tithonic rays can scarcely yet be said to have been made. Each of these comprises rays differing in constitution, and differing in refrangibility, and, doubtless, to each one specific effects are due; thus, it appears from the experiments of Daguerre and Neipce, that the tithonic rays have the quality of changing the constitution of resinous bodies. There is, indeed, scarcely one of the productions of vegetable life which is not characterized by the facility with which it undergoes mutations under the influence of these principles; thus, wax, which is yellow, bleaches rapidly; chlorophyl, which is green, turns gray; the volatile oils harden, as is often observed; the oil of oranges turns red; guiacum, from a yellowish brown, turns green or blue; the colouring principle of most petals is destroyed; for example, the beautiful red of carthamus, and the purple of violets, is rapidly bleached; woody fibre also undergoes a disintegration, and, indeed, there is scarcely a vegetable product on which some one of the various radiations does not leave a specific impression. The extensive series of experiments made by Sir J. Herschel on the

action of light on different vegetable colours serves to prove that there is scarcely a ray which is not implicated in these processes; sometimes it is the red, sometimes the yellow, sometimes the blue. So, too, with rays differing specifically, the rays of heat of different refrangibilities, and the tithonic rays.

397. In the lower orders of animal life, the controlling influence of the same agents which are thus so active in the vegetable world is well marked; the movements of the polygastric infusorials are well known to be directed, to a certain extent, by light. There seems to be a diffused sensibility to that agent possessed by the entire surface of these beings. Comparative anatomists have traced how, from this, which is the obscurest development of specific nervous action, the nervous material begins to be collected in centres, and locomotive or respiratory ganglia make their appearance. In the case of insects, the metamorphoses of which are open to our inspection, as they pass successively through the larva and pupa states, and finally reach their imago, or perfect condition, so far as the nervous system is concerned, all the transitions tend to a concentration; the development of a new instinct, or the production of increased locomotive power, is at once expressed by changes in the magnitude or position of the nervous ganglions, or their connecting cords. It is true, that as soon as this concentration appears, it brings with it new qualities, and, in the more elevated orders of life, one of the great functions of the nervous system seems to be the establishing of sympathetic connexions between organs which are carrying on processes that are essentially different; between the digestive, the respiratory, the secreting apparatus. Among plants, this high state of centralized organization is not required; the sap, driven, as we have seen, by mere mechanical forces, makes its way from the spongioles to the leaves, and then commences its descent. This circulatory movement is the only bond of union between the various parts of a vegetable system. For this reason, therefore, botanists are fully justified in their assertion, that a tree is not a simple individual, but rather a colony of individuals. It possesses no interior nervous system, the office of which is to bring into connexion parts that are distant, because the type upon which it is constructed requires no such machinery.

398. But there are considerations which enable us to trace analogies between the mode of action of the nervous principle, even in the most elaborate forms, with those more obscure, which belong to the vegetable world. As has been fully proved by physiologists, all the nerves of special sensation take their origin amid the minute ramifications of bloodvessels; and it is the changes which occur in the circulating fluid which impress a specific action on the nervous terminations—an action at once transmitted to the centres, and there disposed of. Thus, by the heart, venous blood, from which the elements of carbonic acid have to be expelled, is thrown into the minute ramification of the pulmonary artery, and there, coming in contact with the terminal fibres of the *par vagum*, it impresses upon them an influence—an influence which is in an instant propagated to the respiratory nervous centres, and there is expended in producing a reflex effect. Through the proper nervous channels, the diaphragm, the intercostals, and the various muscles engaged in respiration, are put in action; the capacity of the chest increases; a few cubic inches of atmospheric air enter the trachea, and fill the

larger bronchial tubes. In an instant, by diffusion (Ap., 50), the oxygen of this air leaves its nitrogen, which in these circumstances is a more slowly-diffusing gas, finds its way to the minutest air-cells, passes in an instant (Ap., 53) through the thin vascular coats, interchanges with carbonic acid, and the act of arterialization is complete. The arterialization of the blood is, therefore, nothing more than an ordinary chemical process, and the nervous agency which has been brought into action is for no other end than the production of a mechanical effect; for, though the act of respiration is eventually carried on under the operation of chemical laws, mechanical movements have to precede. A division of those nerves puts an end to the process, not because respiration is the result of a vital, a nervous, or any other such agency, but simply because the necessary movements which end in the introduction of oxygen gas can no longer be accomplished.

399. Now let us turn to the vegetable kingdom. As long as the sun is above the horizon, his beams, impinging upon everything, occasion an elevation of temperature. The various objects exposed—the leaves of trees, the grass, and the surface of the ground—become warm. Participating in this elevation of temperature, the atmospheric air has its capacity for vapour increased, and that transpiration of steam which is copiously going forward from the exposed vegetable surfaces, from the soil, and from water, raises the dew point. But on the going down of the sun, and even before the close of day, a reverse action begins to be established. Radiation into space is accomplished, the temperature of the ground and of the leaves begins to fall, and by-and-by the dew point is reached, and now drops of water form on those organs, and under its influence they begin to recover the parching effect of the preceding day. The reduction of the atmospheric temperature down towards the dew point, thus brought about by the conjoint agency of the ground and the exposed parts of plants, in an instant puts a check upon evaporation from the leaves; this, in its turn, reacts upon the ascent of the sap; it is like putting the cover over the burner of a spirit lamp; evaporation from the reservoir is stopped, and simultaneously, also, the current along the capillary fibres of cotton is checked. With this check upon the ascent of the sap, that store of carbonic acid gas which exists in the soil, and which arises from the decay of humus, or from artificial manures, is also preserved; carbonic acid which, during the day, comes up to the leaves with the circulating juice, and is decomposed by solar light. But, now, if a cloud intervenes, or the sky at night be uniformly obscured, no radiation can be accomplished, no reduction of temperature takes place, the dew point is never reached, and evaporation goes on from the leaves, and carbonic acid gas, drawn up from the spongioles, is expended. The reflex action of that radiant heat which came from the sun during the day is cut off, its passage outward into the regions of space arrested, and the dependant physiological phenomena cease to be performed. There is an analogy of effect between the temporary action of an overhanging canopy of clouds at night on the vegetable world, and the permanent injury which results to an animal when the *par vagum*, one of the offices of which is, at proper moments, to introduce a measured quantity of oxygen gas into the system, is divided.

400. Where, then, do these things carry our reflections? What are the elevated

ideas they bring before us? Do they not show that the great spaces of the universe are not empty solitudes, in which there only reside mechanical forces, in which only the influences of gravitation and projectile action occur? Do they not teach us, that wherever a ray of light can pass, there is the capability for organization and life? And of those innumerable stars which we see at night, some of which are giving forth rays of one, and some of another colour, and a multitude of double stars, which furnish complementary lights to their attendant planets, who can tell what multiplied results these things impress on the world of organization? In our reflections on the constitution of the universe, though the beautiful perfection of its mechanism may excite our wonder, do not these views of its capability for organization, of the constant presence of light, the parent of life, call for our unbounded admiration? Instead of regarding the interplanetary spaces as a great vacuum, a desolate solitude, they rise before us as regions filled with active forces, and ready to put on or to communicate movement and life.

401. Mental volitions are executed by muscular movements arising from the passage of some agent along the nerves, an agent which does not require any perceptible time for its transit, but, within the spaces which we have under consideration, seems to act instantaneously—a volition originates, and contemporaneously a motion is accomplished. The speed with which the different radiant principles are propagated through the ether, rivals the speed of nervous movement; spaces such as those which we ordinarily have to do with are passed over in an inconceivably short period of time.

402. It is not without abundant reason that we are thus led to describe the solar radiation as discharging the part of a vegetable nervous agent, which simulates, in many of its operations, the nervous principle of animals. Out of a limited number of ponderable substances, such as carbon, hydrogen, nitrogen, oxygen, and a few others, all kinds of organized structures are formed, but then there is an extensive machinery to collate and group together these different bodies. Light, in itself, can produce as many different effects as there are possible combinations of colour, for each one of its rays has peculiar powers of its own, and it is also attended by other invisible and imponderable principles which have their modes of action. An organized structure of a given kind is, therefore, the result of the operation of many of these forces, and is an expression of their aggregate action. In the full development of a perfect tree there has been expended a measured quantity of forces, of light, or of heat, and the organized mass, as it stands before us, the product of those forces, is the resultant of millions of vibrations of the luminiferous ether which have acted upon ponderable atoms; vibrations which have stood in a certain relation to each other, as the symmetry of the vegetable parts indicates. In the operations of human agency, something of the same, though of a grosser kind, may be seen. We have not, it is true, the power of calling into existence, or of determining in an enduring shape, or of giving an imbodied form to material atoms; but in the same manner that Nature, operating through ethereal undulations, creates the various forms of vegetable life, there has been committed to us a similar control over those grosser undulations which move in the atmospheric air. The imagination, the genius of the great masters of music, have already grouped together combinations of these waves, which are destined to an earthly immortality; combinations which, when

once heard, leave their indelible impression on the memory, and are to us an imbodiment of symmetry and harmony. These ideal creations, which exist only for the mind, are analogous, in very many points of view, to those more tangible creations which are formed by ethereal waves, and which Nature has reserved in her own hands. The symmetrical or beautiful forms which are transmitted to the brain by the eye, appeal at last to that same, that common principle, which receives melodious or harmonious sounds transmitted by the ear; and the creations of human genius, whether they be expressed in the language of music or of painting, whether they are heard in the cathedral, or seen in the canvass of Claude Lorraine, give us pleasure, because their final impression is made on a mathematical organ which is so constructed as to appreciate whatever is symmetrical in position, whatever is graceful in figure, whatever is harmonious in movement.

403. From this point of view, therefore, I look upon the vegetable world as an imbodiment of the action of ethereal agents. A tree, when covered with blossoms in spring, or loaded with fruit in autumn, is a resultant of the play of those active forces which have been emitted by the sun, an expression of what has been done by vibratory movements operating on ponderable molecules. As soon as the young plant has put its ascending axis above the ground and exposed itself to the solar beam, growth rapidly begins to take place, and organized matter to be condensed from the air; and now a green colour is developed, and the stem elongates, and leaves are put forth. At the proper epoch the reproductive organs are evolved and flowers appear, and by the end of its annual period the plant has laid up a store of nutritive matter for the ensuing spring, or, if such be its habit, has provided for the germs it has called into existence. In carrying forward all those multiplied operations which have ended in these events, its leaves and its stem have gone upward in search of the light—light which has symmetrically arranged their parts and furnished their substance. But these general views are far from giving us an accurate idea of the forces which have been expended, or the motions which have been executed in producing the result we contemplate. An exogenous forest tree, from its magnitude, rising, perhaps, a hundred feet above the ground, and spreading its branches over hundreds of square yards, may impress us with a sense of sublimity; a section of its stem might assure us that it had lived for a thousand years, and its total weight could only be expressed by tons. An object like this may, indeed, call forth our admiration, but that admiration is expanded into astonishment when we come to consider minutely the circumstances which have been involved in producing the result. If we conceive a single second of time—the beat of a pendulum—divided off into a million of equal parts, and each one of these inconceivably brief periods divided again into a million of other equal parts, a wave of yellow light during one of these last small intervals has vibrated five hundred and thirty-five times. And now that yellow light is the agent which has been mainly involved in building up the parts of the tree, in fabricating its various structures, and during every one of a thousand summers, from sunrise to sunset, the busy rays have been carrying on their operation—who, then, can conceive, when, in the billionth of a second, such enormous numbers of movements are accomplished, how many have been spent in erecting an

aged forest oak! Who also can conceive the total amount of force employed from century to century in arranging the vegetation of the surface of the globe!

404. I therefore regard a planetary body like the earth, in its orbitual revolution round the sun, as a predetermined focal centre on which the emanations of that star shall be expended; first, in producing vegetable organization, and, finally, in lending their aid to the evolution of animal intellect. The forces which NEWTON revealed as urging such a body forward, or causing it to glide in its elliptic path, appear only as an incidental, though essential part of the mechanism of the universe, the interest of which disappears in that higher interest which must attach to whatever stands in intimate connexion with organization and vitality. Those many-coloured luminous wavelets which are ceaselessly crossing the interplanetary spaces, go forward on an appointed errand, and sooner or later discharge their final task; nor are the planets in the solar system a colony of opaque globes, rotating, without purpose or end, around the central attractive mass. The solar system is an orb of movement and light, full of vibrations of every tint visible and invisible, and which here and there envelops and enshrouds revolving points of organization and life.

GENERAL APPENDIX,

CONTAINING THE

EXPERIMENTAL DOCUMENTS.

EXPERIMENTS, &c.

CHAPTER I.

EXPERIMENTS MADE TO DETERMINE WHETHER LIGHT EXHIBITS ANY MAGNETIC ACTION.

(*From the Journal of the Franklin Institute for February*, 1835.)

CONTENTS: *Character of the Sky in Virginia.—Examination of Mr. Christie's Experiment.—Needles not affected by the Violet Rays.—No Reaction between a Magnet or Voltaic Currents and Light.*

1. "THE more refrangible rays of light are said to possess the property of rendering iron and steel magnetic. The existence of this property was first asserted by Dr. Morichini, of Rome. Other observers subsequently failed in obtaining the same results; but in the year 1825, the fact appeared to be decisively established by the learned and accomplished Mrs. Somerville, in an essay published in the Transactions of the Royal Society. In her experiments, sewing needles were rendered magnetic by exposure for two hours to the violet ray, and the magnetic virtue was communicated in still shorter time when the violet rays were concentrated by means of a lens. The indigo rays were found to possess a magnetizing power almost to the same extent as the violet, and it was observed, though in a less degree, in the blue and green rays. It is wanting in the yellow, orange, and red. Needles were likewise rendered magnetic by the sun's rays transmitted through green and blue glass. These results have been verified by M. Zantedeschi, of Pavia (*Bibl. Univ.* for May, 1829), but their accuracy has been doubted by Messrs. Riess and Moser, who consider that the means employed by Mrs. Somerville for ascertaining the magnetic state of the needles were not sufficiently exact. They found the oscillation of needles to be wholly unaffected by exposure to the prismatic colours. (*Brewster's Journal*, ii., p. 225, N. S.) This must still be regarded, therefore, as one of the disputed points in science."—(*Turner.*)

2. It has been supposed that this disparity of results arose entirely from local circumstances. A hazy atmosphere, such as is met with in the northern and middle countries of Europe, might perhaps influence, in some manner, this peculiar property of light, when the clearer sky of Italy allowed an opportunity of making the experiment. Some, indeed, have thought that the observers, who were said to have verified the original results of the Italian philosopher, were themselves deceived in not having previously ascertained the magnetic state of the needles they used. That mistakes of this and a similar kind are easily made, will appear in the course of this communication.

3. During the past summer, I have attempted to satisfy myself whether the more refrangible rays really exert any magnetic influence; and happening to reside in the south

of Virginia, upon the same parallel of latitude as Tunis and the more northerly African kingdoms, I thought the situation too favourable to suffer such an opportunity to pass without endeavouring to gain some decisive information on this contested point.

4. The sky of that part of Virginia is not, however, so bright as might be expected. When unclouded, it is of a clearer blue than the sky of England, and, I think, rather darker, approaching, by several shades, nearer to the tint of Prussian blue than that azure colour which it has in the latter country. During a residence of twelve months, I saw it twice of that intense complexion which they say it has in the tropical regions. The moon appeared globose, and her centre to bulge out like a ball, and the planet Venus might be fancied to exhibit a well-formed crescent. The air passes from great moisture to dryness with rapidity; a good barometer is seldom at rest. The temperature of the sunbeam is sometimes 132 F., but the siphon barometer seldom varies more than one inch. Clouds form and rise with great rapidity, the seasons are uncertain, and the atmosphere is often deformed with a mistiness that obscures distinct vision. But the dense clouds that gather in the west of an evening emulate the beauty of a sunset on the Atlantic Ocean.

5. I am thus particular in describing the state of the atmosphere in which these experiments were made, because much stress has been laid upon that circumstance, as materially affecting the results of magnetic action of the solar ray. And, as the experiments now to be detailed lead to a negative decision, it is well to explain under what circumstances they were performed, that those who have come to an opposite conclusion may have an opportunity of pointing out whether it is really owing to the state of the atmosphere.

6. In the year 1824, Mr. Christie found that a needle six inches long, contained in a brass compass box with a glass cover, suspended by a hair, and made to vibrate, alternately shaded and exposed to the sun, came to rest much sooner in the latter than in the former case. That this was not occasioned by an increase of temperature, was proved by the needle vibrating more rapidly when its temperature was raised by other means.

7. In repeating this experiment, I very quickly found that it depended, in a great measure, on the nature of the suspension of the needle, and its position with respect to the incident light, what results would be obtained. If the needle was suspended on a point or by a thread, without torsion, both the time and the number of vibrations were the same, whether the needle was exposed to the sunbeam or not. But, if the needle was suspended by a hair or other organic substance, having torsion, the sunbeam would occasion a degree of twist in the hair on its first exposure to light; and if the direction of that twist happened to coincide with the direction of the needle's motion, of course the momentum of the needle was increased, and the vibrations continued longer. A needle which vibrated forty-four times in one minute, would occasionally, owing to this cause, vibrate nearly forty-six when suspended by a hair; but if by a silk fibre, its vibrations were always forty-four, the first arc of vibration being in every instance 40°. That this action was due to a twist of the hair, as a hygrometric body, I ascertained by means of a simple arrangement. To a brass cross-piece, A (*fig.* 1, *pl.* 1), supported

on a stand by two pillars, B C, a hair, *e*, was fastened, about the point A; its other extremity, *g*, being fastened to a whalebone spring, D, by a thread, *f*; the extremity, *g*, of the hair, being bent by the knot on one side, served as an index. On exposing the instrument to the sunbeam, the little index, *g*, immediately moved, sometimes more than a semicircle.

8. Thinking to obtain more decisive effects, I concentrated the sunbeam with a lens on the south pole of the suspended needle, and found that the needle was thrown into a rapid, tremulous motion. But here the hot air ascending from the needle, acts upon it as upon the sail of a windmill; and the same effect ought to take place, to a certain extent, on simple exposure of the half of a vibrating needle to direct light. But I found that a needle suspended in the vacuum of an air-pump by a thread without torsion, is in no way affected by exposure to solar light.

Ex.	Length of needle in inches.	No. of vibrations in shade.	In the sunbeam.
1	$1\frac{1}{2}$ sewing needle	44	44
2	2 blue spring	32	32
3	$3\frac{1}{8}$ do.	26	26
Therm. in the sun, 109 to 105.—Barom., 29.3.—Duration of Exp., 60″			

9. It is said expressly in the account of Christie's experiment, that the needle was contained in a brass compass box. It might have been that electrical currents were excited in that box, which was the cause of the derangement in question; I therefore vibrated a needle, under similar circumstances, with the same result as above stated. I should mention that this was done in a solid cylinder or ring of brass, without any seam or soldered junction; but as compass boxes are generally made of sheet brass, with a soldered seam in the side, it was barely possible that the fine line of solder acted with the brass, as a thermo-electric couple, capable of excitation by the warmth of the sunbeam. I therefore made a compound cylinder of copper and zinc, *c z* (*fig.* 2, *pl.* 1), the edges of which, at *a* and *b*, were neatly soldered, the junction *b* being highly polished, and that at *a* blackened. The needle was suspended in an exhausted receiver, by a silk fibre, *g*, and a ray of light, *c d*, coming from an aperture half an inch wide in the shutter, fell upon the junction. The needle used in this experiment was of watch-spring; its first vibration was performed in an arc of forty degrees, and when the compound cylinder was taken away, it made thirty-two vibrations in sixty seconds in vacuo. On placing it concentrically with the compound cylinder, and suffering the ray to impinge on the polished junction the moment that the arc of vibration had become forty degrees, the number of oscillations in one minute was carefully observed; six experiments gave severally the number thirty-two. On turning the blackened junction to the light, the result was still thirty-two; and on substituting the solid brass cylinder, three consecutive trials gave thirty-two. The thermometer stood in the sunbeam at 103; the barometer at 28.8.

10. By some this magnetic action of light has been attributed to the violet or more refrangible rays only. A needle made of watch spring, about four inches long, which, in an exhausted receiver, suspended by a filament of silk, exhibited no polarity, had one half of it exposed to the violet ray, cast by an equi-angular prism of flint glass. This ray

was separated from the others by passing through a slit in a metallic screen, and half the needle shielded from its action by a piece of paper. After two hours' exposure to the sun, it was suspended again in the exhausted receiver, but still showed no token of polarity; it was then exposed to the other rays successively, with the same result. The needle was now slightly touched, and, slowly vibrating, arranged itself along the magnetic meridian. The first vibration was performed in a semicircular arc, and the number of vibrations performed during one hundred oscillations of a seconds' pendulum was twenty-seven. But, after four hours' exposure to the violet ray, as before, no evidence of any change, either increasing or diminishing the number of oscillations, could be gained. A column of violet light, passing through a disk of stained glass, was concentrated on one end of a sewing needle by means of a lens, without producing any change in the number of vibrations it made in one minute. This needle, on some occasions, however, would give unequal results; when its first vibration was performed in a semicircle, the number varied from forty-one to forty-three in sixty seconds. On vibrating it in vacuo, its results uniformly gave the latter number very nearly.

11. The position of the needle to the incident ray is not of any consequence, whether it receives it obliquely in the direction of the light or across it. If soft iron be substituted for steel, the results are still negative, even if the needle be arranged in the magnetic meridian, the line of dip, or any other position. I therefore come to the conclusion that the violet ray, as developed by a prism of English flint glass, possesses no influence on the magnetic needle, and all the other rays are equally inert.

12. But, as Mrs. Somerville found that a needle placed under a piece of glass or blue riband, having half its length protected by paper, became, in a short time, magnetic, I tried the same experiment, but, in every instance, failed in making the needle magnetic. When suspended by a silk fibre in vacuo, needles showed no disposition to arrange themselves in any particular line, and when they came to rest, they were found cutting the magnetic meridian at every angle, although the temperature of the sunbeam to which they had been exposed on one occasion was 124° F. Great care was taken to ascertain the previous non-magnetic state of the needles, and they were suspended by a fibre without torsion. To ascertain whether anything was due to the nature of the medium, I substituted prisms of water, alcohol, spirit of turpentine, and other essential oils, with the same results.

13. These are the means by which, it is said, the magnetism of light was first discovered; but there are much more delicate methods of detecting such an action if it existed, and to them, in the next place, I resorted. For, if the violet or other rays of light exercise an influence on the magnetic needle, that action must be mutual between them, and the light, in its turn, should suffer a derangement. To ascertain this, I admitted a divergent beam of light through a hole in the shutter of a dark room; the cone of luminous matter at its apex was about $\frac{1}{10}$th of an inch in diameter, and a hair or other filament held in it exhibited the phenomena of diffraction, the colours being received into the eye by a lens. Across this beam a silver wire was adjusted, each of its extremities connected with cups of mercury, which communicated with the poles of a voltaic battery. It was expected that, if there was any action between a magnetic fila-

ment and light, some derangement would be seen in the diffracted fringes when the current passed; but none such was observable.

14. Again, two wires were so adjusted that they could be made to approach or recede from each other by a screw movement, and voltaic currents be passed in either direction, up or down them, conjointly or separately; when they were within the $\frac{1}{150}$th part of an inch, the fringes they produced were very perceptible, but the passage of the current caused no alteration whatever. These experiments were also repeated in homogeneous light, with the same results.

15. A hollow prism, *a b* (*fig.* 3, *pl.* 1), for containing transparent but imperfectly conducting liquids, was traversed by a voltaic current from the battery, *c z*, by means of platina wires, *e e*, one extremity of each of which went to the bottom of the liquid, and the other dipped into the mercurial cups of communication, *d d*. A ray of light, *f*, passed through the prism, and was refracted on the screen, *g l*, or viewed by a glass. Some of those lines which traverse the spectrum were at times visible; but neither these nor the spectrum itself suffered any perceptible alteration during the passage of the current.

16. Some philosophers have pointed out the different degrees of temperature which Sir W. Herschel detected in different parts of the prismatic spectrum, as analogous to that increase of temperature which takes place in the cells of the voltaic battery, as we proceed from the negative to the positive pole. Hence, they have supposed that each end of the spectrum was in an opposite electrical state. With the analogy I have nothing to do; but to ascertain whether the deduction from it is founded in fact, I made this experiment. I received into a tube, *a b* (*fig.* 4, *pl.* 1), filled with acidulated water, the whole of the prismatic spectrum cast by an equiangular prism of English flint glass, in that part, *c*, which was covered by the violet ray; I immersed one wire, *e e e*, of a galvanometer, arranging the other wire, *f f*, in that part, *d*, which received the red ray. Alternately placing and withdrawing the arrangement from this position, it was to be expected that either a continuous current or a wave of electricity would pass along the galvanometer wires, producing either a permanent or transient disturbance. I was not able, however, to notice such an effect; and suspecting that, if any electricity was developed, it might be of such low intensity as to be unable to pass through acidulated water, I repeated the experiment, using, instead of the tube of water, a piece of polished tin, in the shape of a parallelogram, three inches long and one broad, to each end of which the galvanometer wires were soldered; but the results were still negative.

17. The general termination of these experiments would lead us to suppose, that if there be any reaction between galvanic or magnetic currents and solar light, they are of such a nature as to forbid repetition in latitudes as far south as 35°.

CHAPTER II.

OF THE TIDAL MOTIONS OF MOVABLE ELECTRIC CONDUCTORS.

(*From the Journal of the Franklin Institute for January,* 1836.)

CONTENTS: *Description of the Phenomenon.—The Polar Wires act as Centres of Attraction.—They produce Tides.—Cause of the Oscillations.—Cause of the Spiral Motions.*

18. "DANS d'autres circonstances on observe encore au milieu des masses liquides, des mouvemens singuliers qu'il est excessivement difficile de décrire, tant ils sont nombreux et changeans. Je vais essayer d'en donner une idée, en remarquant toutefois, qu'après avoir fait de nombreuses expériences sur ce sujet, il m'a été impossible d'en saisir la loi."—(*Pouillet.*)

19. The singular movements here spoken of by Pouillet have likewise drawn the attention of several other philosophers. Erman and Serullas have both recorded instances of gyratory motion produced in certain bodies, especially mercury, by the contact of others. There is also a similar observation made by some of the earlier chemists respecting camphor. Strange motions of an analogous description are also observed in some liquids under the influence of a voltaic current; these, in the case of mercury, have been particularly studied by Sir J. Herschel, who obtained several remarkable notices respecting them; they are, however, so far as I am informed, as yet without explanation.

20. If into a watch-glass or shallow capsule, as *a a* (*fig.* 5, *pl.* 1), fifty or sixty grains of mercury be poured, and over that as much water, acidulated with sulphuric acid, as is sufficient to cover the surface of the mercury, and the positive and negative wires of a battery of twenty or thirty plates, arranged as indicated, the mercury being in contact with the negative pole, and the positive pole being plunged into the water at a short distance from it, currents are produced both in the water and in the mercury. Supposing the power of the battery sufficient, the same effect takes place on removing the negative wire out of the mercury into the water; but if the positive wire is in contact with the mercury, and the negative with the water, there is no motion at all, or, at most, the mercury only curls itself up into an elongated figure.

21. This motion varies according to several circumstances, but chiefly from the position of the two wires. 1st. If the wires be on opposite sides of the mercury, the metal instantaneously elongates, and currents also are seen playing in the water. 2d. If the negative wire be introduced into the centre of the metallic globule, and the positive wire be brought on one side, the mercury will bulge out *elliptically* at both sides, nearest and farthest from the positive pole; and by regulating the force of the battery, either by changing the number of the plates, or altering the strength of the solution acting on them, the experiment may be so managed that no motion shall ensue in the mercury

after this elliptical bulging is effected; but now, if the negative wire is cautiously raised from its position, so as to be just out of contact with the surface of the metal, the mercury is immediately convulsed, and its whole surface covered with circular waves. On lowering the negative wire to its former position, and advancing the positive, the moment it comes to the edge of the mercurial ellipsoid, the most intense convulsions are produced, which increase until contact of the mercury and wire is obtained. 3d. If the two wires form a kind of triangle with the globule, it turns upon itself.

22. At the same time that these movements are going on, the surface of the water is ploughed by gentle currents, exactly resembling those produced by a breath from a blowpipe, directed slantingly across the surface.

23. In proceeding to give an explanation of these motions, I shall not follow the analytical course of experiment used in my researches, but commence with those principles on which a true explanation is founded.

24. It has long been known that the elements of compound substances were held together in virtue of an affinity among themselves. Sir H. Davy, Berzelius, and other chemists, were led to suspect that this was due to the electric condition of those elements, and pursuing this hypothesis in its details, several brilliant discoveries were made, which ultimately changed the face of the science. Apart, however, from all hypothetical reasoning, it was found that the poles of a voltaic battery had the power of influencing the atomic constitution of bodies, so as to be able to hold all chemical combination under control. This remarkable effect was imputed to the electrical attraction and repulsion of the battery; but a battery which is competent to the rapid decomposition of water, and even the reduction of potash, is found to give exceedingly faint traces of any electro-dynamic effect, being unable to cause the divergence of a delicate gold leaf electrometer, or affect the indications of a torsion balance. In the course of certain experiments, I had occasion to notice that this effect, as to intensity, is entirely regulated by the medium in which the experiment is made; as, for instance, a thin lamina of air or gaseous matter is nearly a perfect non-conductor to electricity of low intensity, but a mass of water offers no such resistance. I hoped, therefore, that though I might not be able to exhibit the attraction of a polar wire for a suspended needle in the Coulomb balance, such an effect might ensue if the experiment was made with the apparatus plunged in another atmosphere, the conducting power of which differed from that in which we live. For the conducting power of a medium has no relation either to its cohesion or its chemical properties, and it did not appear improbable that one might be found, which, though it should not interfere with the freedom of motion of a wire plunged in it, its conducting power, in relation to electricities of very low intensity, might exhibit those effects in a more elevated point of view.

25. To illustrate this reasoning, I took a platina wire, *a c* (*fig.* 6, *pl.* 1), two inches in length, and suspended it by a raw silk thread from a stand, *b b*, into a vessel filled with acidulated water, as high as *d d*. The needle was so arranged that when it hung with freedom, it was about one fourth of an inch distant from the extremities of two platina-pointed wires, *p n*, which entered the vessel on opposite sides, and could be made to communicate at will with the opposite poles of a battery. Now the wire *p* being pos-

itive, and n negative, the extremity a of the suspended needle would be negative, and the extremity c positive by induction. The conjoined effort of the forces thus brought to bear on the needle, acting on its opposite extremities in opposite directions, would solicit it to move on its axis, the extremity a in the direction n' (*fig* 7), and the extremity c in the direction p'; the line of rest would be as expressed by the dots in the figure, and slow oscillations should take place on either side of that line if the density or other properties of the medium permitted.

26. The experiment was thus tried, and, to prevent any derangement from hygrometric twist of the silk, the needle was hung on a glass thread, of sufficient length to reach above the surface of the water, and there attached to the silk; on passing the current of forty-five pairs of four inch plates, the needle immediately moved, and after two or three oscillations, took its position of rest; on being moved to the opposite side of the polar wire, an opposite motion ensued until the same position was gained. During this movement, gas was freely liberated from the extremities of the polar wires, and also from both ends of the needle, it hindered considerably that freedom of motion which I had hoped for in observing the oscillations. The experiment was also varied by terminating the polar wires with plates of platina, with a view of increasing the effect; the needle was also suspended in pyroligneous ether, and the attractive power of the same battery, newly charged, was very marked; it was not so observable in alcohol, and still less in muriatic acid; in ammonia, though only one end of the needle appeared to evolve gas, it was not so obedient to the attractive force. These circumstances indicate that the phenomena of motion, as here exhibited, have not their origin in any magnetic action produced either by the disturbance of the earth or the passage of the voltaic currents. Magnetic action, to be complete, requires that the bodies along which currents are passing should be possessed of high conducting power; hence a thermal current, whose tension is almost extinct, is still capable of producing a powerful effect on a suspended needle. A current capable of producing a given deviation when moving along metallic wires, would meet with resistance in passing through water; and alcohol or ether would forbid its passage. It is, moreover, impossible to produce any visible effect on the platina wire of this arrangement by the action of a single pair, even possessing extensive surface, though the same pair, if cut into lesser plates, and arranged for the production of a current of greater tension, immediately causes the movement here described. Dr. Faraday has stated, in his recent researches on this point, that there is not any proof that the poles of a battery do exercise any power of attraction or repulsion (*Bache's Turner's Chem.*, p. 102; idem, 108); but that opinion would appear to be inconsistent with the fact—there must be an accumulation of tension on an electrode, if the medium which separates it from its fellow is not so good a conductor as the liquid filling the cells of the battery; and experiment warrants this conclusion.

27. The principles here laid down also indicate the construction of a galvanometer which I have recently fitted up. It is intended to exhibit, by the torsion of a fine fibre, the force of attraction between the polar wires and the ends of the suspended needle. The obstacle I have observed to the accuracy of the results furnished by it, is due to the development of gas on the polar wires and on the needle.

28. The doctrine which I wish to establish from this experiment is, that though the polar wires are plunged in a conducting medium, and the current is actually passing, yet they still act as centres of attraction. The motions of mercury and other fluids are only exemplifications of this doctrine.

29. When a spheroidal mass of conducting matter is brought in presence of a point of attraction, situated at a distance from its surface, the particles on that surface will be differently affected as their situation in regard to the attracting point varies. Thus, on touching the mercurial globule, named in the first part of this paper, with a negative wire, and introducing into the water a positive platina pole, the globule, which before was spherical, becomes ellipsoidal, two tides are formed upon it, one directly opposite the positive wire, and the other 180 degrees from it; meanwhile there is an ebb in those regions which are situate a quadrant from the point of attraction. If the positive wire is made to revolve round the globule, both tides move, always keeping the same relative position to the point of attraction that they had at first. It only requires the force of the battery to be appropriately moderated to exhibit these phenomena with the utmost rigidness. And as these motions exhibit very nearly, on a small scale, that effect which takes place on an immense scale by the joint action of the SUN and MOON in producing the tides of the ocean, I have given them the name of Tidal motions of movable Conductors.

30. Now the mechanism which produces the change of figure from a sphere to an ellipsoid is sufficiently obvious. We have two forces under consideration: 1st. The cohesion or gravitation of the mercurial particles upon each other; and, 2d. The disturbing force of the polar wire as a centre of attraction. As that disturbing force decreases in a certain ratio, as the distances increase, the mercurial particles on the side, A (*fig.* 8, *pl.* 1), nearest to the polar wire are more attracted by it than those in the centre, C, of the globule, and those in the centre, C, are more attracted than those at F. The particles, therefore, at A rise towards the wire by its direct action, those at F being less solicited towards the centre of the globule than those at E and B; the former recede from that centre, while the latter seek it.

31. It has been observed that a true theoretical tide differs in no respect from a wave. "Suppose a spring tide actually formed on a fluid sphere, and the sun and moon then annihilated, the elevation must sink, pressing the under waters aside, and causing them to rise where they were depressed. The motion will not stop when the surface comes to a level, for the waters arrive at that position with a motion continually accelerated. They therefore pass that position, as a pendulum passes the perpendicular, and will rise as far on the other side, forming a high water where it was low water, and low water where it was high water. And this would go on forever, oscillating in an assignable time, if it were not for the viscidity of the water." Now this theoretical case may be easily shown, for on approaching the positive wire towards the globule of mercury, a particular position will be gained, at which contact will take place between the protuberant tide on the mercury and the wire. In that moment the cause of attraction is annihilated, the whole current of electricity now passes along perfect conductors, hence fulfilling the supposed case of an actual annihilation of the sun and moon at the time of spring tide. And the same reasoning that held in one case, equally applies in

the other; the mercurial tide falls with an accelerated motion, and the line which before was the conjugate axis of the ellipse, now becomes the transverse, a tide being produced at right angles to the former one. But here the strict comparison ends, for, as the mercury ebbs from its protuberant position, the metallic connexion breaks, and the wire is again put in action as a point of attraction; the motion of the ebbing tide is checked; it flows once more; once more the metallic contact is complete, and when the tide falls, it is only to flow again, as long as the battery current passes. Tides take place at right angles to each other, in a series too rapid to be counted, and the whole surface of the mercury is worked into those various and beautiful undulations which have been before referred to.

32. In endeavouring to ascertain the true cause of these phenomena, the French philosophers were, I believe, the first to observe motions in the water or other liquid of communication, as if a gentle wind played over its surface, bearing light bodies in its vortex. The explanation of these appearances I here add, because no one as yet has given it, and it affords an illustration of certain propositions delivered by Sir I. Newton, in his Principia, concerning the doctrine of pulses in elastic fluids.

33. We have hitherto been considering a globule of mercury as a substance mathematically fluid. Such, however, in effect, it is not; the water in contact with it possesses those properties in a much more eminent degree, so that, in comparison with it, the mercury may be regarded as a solid resisting obstacle. Now, about a year ago, I showed that when a voltaic current passes through a system such as this of mercury and water, the capillary pressure on the bounding surface is changed; but, if the attraction of the wire which is introduced into the water, and which is the ultimate cause of this derangement, decreases in a duplicate ratio, it follows that this disturbance of pressure obtains only to a limited extent on the surface of the mercury; or, in other words, the excess of pressure produced by a voltaic current is not spent equally on all parts of the mercurial surface, but those which are adjacent to the positive polar wire are more affected than those at a distance. Newton has shown (*Principia*, v. ii., b. ii., pr. 41), that if the particles of a fluid do not lie in a right line, a pressure propagated through that fluid will not be in a rectilineal direction, but the particles that are obliquely posited have a tendency to be urged out of their position. So the particles *a a a a* (*fig.* 9, *pl.* 1), pressing on the particles *d b*, which stand obliquely to them by reason of the shape of the mass of mercury *g*, have a tendency to be urged from their places towards *e* and *c* respectively, and the motion thus produced in a fluid diverges from a rectilinear progress into the unmoved spaces; and such a pressure taking effect on a liquid free to move, continually returns the moving particles to their first position, after making them describe an elliptical orbit.

34. It has been remarked that the basis on which this explanation essentially rests is, that a wire, from which an electric current passes, acts still as a point of attraction; an effect which involves the conducting and other electric properties of the system on which the experiment is tried. Hence we gain an insight into the cause of the paralysis of these motions by the addition of certain substances; the spiral motions going on over the surface of the water have these explanations complicated with another consideration, the figure of the mercurial mass.

CHAPTER III.

ON THE INTERSTITIAL MOVEMENTS WHICH TAKE PLACE AMONG THE PARTICLES OF BODIES.

(*From the Journal of the Franklin Institute for March and July*, 1836.)

CONTENTS: *Of the Mode of Passage of Liquids through Pores.—Endosmosis.—Percolation through Gum Lac, Gold Leaf, Mica, etc.—Slow Motions in the Parts of Solid Bodies, as in Silver Coins.—Percolation through India Rubber.—Conditions of Equilibrium.—Percolation through Masses of Water.—Percolation through excessively thin Films of Water, as Soap Bubbles.—Analysis of Gas on the Exterior and in the Interior of the Soap Bubble.—General Law of the Phenomenon deduced.*

35. THE interstices which exist in a great variety of bodies may be looked upon as an extensive system of capillary tubes, into which we should be prepared to expect that bodies of all kinds might pass. A drop of water placed upon a porous stone or a piece of chalk, sinks into it rapidly, but the value of the observation is lost, because it is common. If that water contained a colouring matter, we should find that, in sinking into the chalk, the colour would be left on the surface. But here, again, commonplace principles dictate a ready answer: the interstices of the chalk may be supposed to be too small to admit the colouring matter to pass, or perhaps some incongruity of shape might afford a barrier; yet how, upon these principles, shall we explain that mercury and other bodies remain unmoved upon the porous mass, and show no ability to go through it, when they will pass with readiness into the densest and closest substances, as gold? No principle of coaptation will explain why quicksilver will not rise in a tube of glass, or why water rises at all. We are induced at once to refer the whole matter to the chemical conditions of the bodies on which we operate, and we quickly infer that fluids do not pass into pores by soaking or leakage, or any such commonplace principle, but that it is an action determined by certain laws that have reference to the condition of each body separately, and their relation to each other. A question, therefore, naturally arises as to the peculiar operation of those pores, and how changes in their position, size, and shape affect the results of their action. A class of these phenomena is quite independent of pores of any sensible size, where no leakage or oozing can be suspected. A piece of sugar dissolving in water diffuses itself into every part of the menstruum. Among those excessively small interstices that exist between the atoms of the water, its particles find a dwelling, where they are sheltered from all those forces that act so energetically on the great masses of matter. Independent of gravity, they move freely in every direction; and, far from settling in those positions to which they might tend from their weight, they are simultaneously and equally found in every portion of the solvent. This condition of things does not indicate a passive state, but would rather teach that a very active and powerful force is in operation, a force that can neutralize the action of gravity and other external agents. It is essential, therefore, clearly to understand the circumstances of

this absorption; it may take place independently of apertures, pores, or vessels; it may take place between gases and gases, gases and vapours, or liquids, or solids, or mutually and indiscriminately among them all.

36. When a liquid rises in a capillary tube, those portions only are under the direct influence of the attractive force of the tube which are nearest to it, the central columns being entirely unaffected. Also, when water jets out through a narrow pipe, it is only those portions that are directly in contact with the sides of the pipe that are subject to its resisting influences, any disturbance which the central particles feel arising indirectly from their cohesion. The same applies in the passage of liquids among pores: the diameter of these pores amounting to a certain size, they will admit a passage without exerting any direct influence. Thus, a pore in a piece of charcoal may suffer a column of water to go through it, without in any wise affecting the central portion of that column, by reason of its size; but should the diameter of the pore be made to decrease, it is obvious a limit might finally be reached, where every particle that passed should come under the direct influence of the physical force exerted by the pore, and none pass by mere leakage or oozing.

37. This leads us to consider the different effects that may ensue when the same liquid or gas passes through pores of various sizes in the same solid. An example may, perhaps, illustrate the results: The walls of a pore are so constituted as to allow an easy passage of one gas, as oxygen, along them, and afford more or less resistance to another, as nitrogen gas. Now, if we suppose this pore to be of very large size, and atmospheric air to be passing through it, little or no change will happen in the constitution of the passing gas, all the internal parts of the current being out of the reach of the walls of the pore; but should the diameter of the pore be reduced to the diameter of an atom of the compound gas, or thereabout, the oxygen, finding little or no resistance, would glide through, and the nitrogen be retained, a perfect decomposition happening. This shows the importance in all investigations relative to Endosmosis; or transit of bodies through pores, of bearing in mind that, when those pores have a certain diameter, the results of experiments made on them are illusive, not representing alone the nature and value of the force exerted by the walls of the pore, but showing effects depending also on the cohesion and other properties of the passing body.

38. These observations apply to those experiments which have been made to illustrate the phenomena of endosmosis by forcing gases through plugs of stucco, which are systems of capillary tubes of large size. Experiments on charcoal, plaster, &c., are also open to the same strictures. Had these only been resorted to, the simplest phenomena of endosmosis could not have been discovered. The disturbance of hydrostatic level, which is so well shown by a sheet of gum elastic, or an animal membrane, cannot be produced by the use of plugs with large pores or systems of capillary tubes.

39. It might at first be expected that, as the diameter of a pore decreased, its indisposition to admit a foreign body would increase; but it is not so: that foreign atom does not insinuate itself in a passive manner, nor does it go through the pore merely because it meets with no resistance. There is an active and very energetic force in play, a force that is even greater than the cohesion of the parts of the pore itself. Hence,

under like circumstances, the smallness of such a pore is no bar to its receiving and transmitting foreign atoms, but very often, in an experimental point of view, is the most favourable condition under which we can study its action without any retarding or complex causes.

40. There are some experimental illustrations of the fact, that closeness of texture is no hinderance to the passage of suitable bodies. I took a narrow glass pipe, about an eighth of an inch in diameter, and dipping one end of it into melted gum lac, expanded thereon a bubble of that substance, by blowing at the other extremity. In this way, after a few trials, the bubble may be made so thin as to be translucent. Such a bubble, with air from the lungs in its interior, being exposed to an atmosphere of ammoniacal gas, allows a free passage to it. A singular change in the appearance of the thin membranous bag takes place during the experiment: from being brown in the thicker parts and whitish in those that are more translucent, it becomes of one uniform flesh colour. Now in this state it may be regarded as one of the most impervious of all resinous bodies, and certainly of them all it has the closest texture; yet, after it has thus been exposed for a short time to ammonia, we find, on passing into its interior a little reddened litmus water, that the gas is present in large quantity, and must, of course, have been transmitted along the pores in the resin.

41. On the top of a tube which contained atmospheric air and a piece of litmus paper, tinged red by the fumes of muriatic acid, I fastened very carefully a piece of gold leaf, two tenths of an inch in diameter, with gum-water, and suffered it to dry. The gold leaf, when examined with a lens by transmitted light, appeared all over of a uniform pea-green colour, nor could any hole or flaw be perceived in it. It was covered with a jar of ammonia on the mercurial trough, the level of the mercury on the inside and the outside being regulated. The gas went through the gold leaf rapidly, and in a very short time the test paper became uniformly blue. On using carbonic acid or sulphuretted hydrogen, the action was very nearly as instantaneous.

42. I split with a lancet a thin plate of Siberian mica, which for the most part appeared of a flame colour, but in places where it was unequally thick, a blue or a red. This plate, when substituted for gold leaf in the last experiment, suffered ammonia to pass through it. A similar plate of sulphate of lime suffered half a cubic inch of carbonic acid to pass through it in forty minutes. Atmospheric air, in all these cases, was on the other side.

43. These permeations, which we have noticed to take place so rapidly under favourable circumstances, occur likewise more slowly in nature. A sea-shell, for instance, deposited in that formation called London clay, in course of time loses its coagulated albumen, then its carbonate of lime, and its other ingredients simultaneously or successively. These are replaced by the sulphuret of iron, by alumina, oxide of iron, &c., which form together a mass of so close a texture, that it can give sparks by collision. Under such circumstances as those which occur along the coast of the Island of Sheppey, a thin plate of carbonate of lime is permeated readily by bisulphuret of iron, so that there is a continued deposition and accumulation of that substance, even in the interior of a thin shell. Hence the production of that immense quantity of fossil shells, which is there

used for the purpose of manufacturing copperas for commerce. Slow motions of the same kind occur when alloys are buried under the ground, or placed in exposed situations; a silver Roman coin has thus been known to part with much of its copper, which formed a species of crystallization on its surface, the *patina* of antiquarians. It is in this way that trinkets of gold, on which small quantities of mercury have fallen, gradually recover their original brilliancy and purity. A number of facts of this kind, showing that even in the most solid of metallic textures motions may take place, might be referred to: these have been well considered by Boyle, in his tract on the languid motions of bodies.

44. Caoutchouc, or gum elastic, is the substance which, of all others, has furnished the most unexceptionable results on studying the phenomena of endosmosis. It, however, at times exerts a synthetic action, which, so far as I know, has not yet been noticed. Having capped an open tube with a thin piece of this substance, and thrown into it 200 measures of hydrogen gas, it was exposed to an atmosphere of 100 measures of oxygen contained in a wider tube, into which it was raised. In eleven days the level in both tubes had considerably risen, and the barrier, which was at first of a blackish colour, became quite white. In sixteen days, the united volume of both gases was only 215 measures; this, on analysis, contained only 14 per cent. of oxygen. It may here be stated that a like mixture of 100 of oxygen and 200 of hydrogen, enclosed together in a tube by the side of the former, had undergone little or no diminution. Now a rough calculation shows that about one thirteenth of the united volume of the gases had been condensed by the membrane into water, the remaining 62·35 parts of oxygen having combined, in some chemical manner, with the substance of the caoutchouc, in the process of bleaching it.

45. This result points out the condensing effect of a membrane, which often, in many arrangements, will have no small influence. Thus, in Dr. Mitchell's experiment, where two bent tubes are screwed together, with a piece of gum elastic between them, the one tube containing oxygen, and the other a double volume of hydrogen, we should be led to expect, from the common theory of endosmosis, that however much the levels in the two tubes might vary relatively to each other, the united volume of the gases ought to remain constant. If the level in the hydrogen tube rose an inch, ought not the level in the oxygen tube to sink an inch? But an appeal to experiment shows that such is by no means the fact; to a certain extent, the volume of gas in the tubes is constantly diminishing; it is not due to leakage into the free atmosphere, between the membrane and the glass that presses it, at least not entirely so; for a part of the gases is condensed by the direct action of the barrier to form water, and the remainder unites chemically with it. In some instances, the action is still more obvious. If a vessel of atmospheric air, the mouth of which is covered by a piece of India-rubber, be immersed in an atmosphere of deutoxide of nitrogen, it will be found that red fumes do not appear in the vessel, nor any other obvious indication of the presence of the deutoxide, but the membrane soon begins to change its colour, and from being diaphanous, becomes of a dirty umber brown, the volumes of the gases on both sides of it diminishing.

46. Into a tube which was covered with India-rubber, standing on the shelf of the

pneumatic trough and exposed to the free atmosphere, I placed 100 measures of atmospheric air and 42 of hydrogen gas, being anxious to see if any passage of the gases would ensue, as the oxygen and hydrogen in the mixture were in nearly a due proportion to form water. Motion at once began, the level of the water in the tube rising for several hours. In the course of a few days, only a trace of hydrogen was discoverable, the remaining gas differing very slightly from atmospheric air. The same was repeated with a tube closed by a serous membrane, kept continually moistened; when all motion appeared at an end, analysis showed that there was only $\frac{1}{16}$th of the whole volume of hydrogen beneath the membrane.

47. These experiments, which were repeated again and again with the same results, establish an important doctrine. If a gas be confined beneath a system of pores, the other extremity of which communicates with another gas, movement will ensue, until the constitution of the gas on both sides of the system is alike. If oxygen and hydrogen be thus placed, they will mutually pass to each other, nor will that motion cease until the resulting compound on both sides of the membrane is the same chemically. This endeavour to an equalization of constitution takes place under all circumstances; it may, perhaps, be partially arrested by the condensing action of the barrier. There are, therefore, two prominent conditions under which the phenomena of endosmosis may be regarded: 1st. During the state of motion. 2d. After an equilibrium is obtained.

48. Aided by this principle, we can explain how mixtures of gases would comport themselves when exposed to free atmospheres, or when shut up in close chambers. The arrangement of (46) will serve as an illustration: here we have a mixture of atmospheric air and hydrogen exposed to the free atmosphere. It is evident that, in pursuance of an attempt to gain an equilibrium, a portion of air from the atmosphere should pass inward through the membrane, and a portion of hydrogen pass out. But as soon as the hydrogen is beyond the outside of the membrane, it is dissipated by aerial currents, or otherwise diffused in the mass of the atmosphere, the condition of equilibrium being in nowise approached to, for so fast as the hydrogen escapes, it is carried off; there being continually hydrogen and atmospheric air on one side of the membrane, and only atmospheric air on the other. Equilibrium, therefore, can only be gained by the entire dissipation of the hydrogen into the free air, and, accordingly, experiment indicates that when that equilibrium is gained, the hydrogen has vanished, and atmospheric air is found on both sides of the membrane. But very different would that action be if the arrangements were included in a close chamber, as beneath a small glass bell; here, when the hydrogen comes out through the membrane, it does not escape, but continually accumulates, and motion ceases, and equilibrium is gained when the relative proportion of the gases outside the membrane is the same as inside. Hydrogen, therefore, in this case, is found on both sides of the barrier.

49. Before proceeding to give an account of the chemical changes that may happen in virtue of the action of capillary forces, it is necessary to remark, that all the analyses of gaseous mixtures, in which oxygen is an element, have been uniformly made by means of binoxide of nitrogen. Living in a climate where no dependance can be placed on the action of an electrical machine, and not possessing Dr. Hare's galvano-ignition apparatus,

I was led by necessity to choose between spongy platina and the binoxide of nitrogen After an experience of some extent in the employment of this gas, it has not appeared to deceive me; it is, indeed, an eligible method in gaseous analysis where oxygen is concerned. The mode of manipulation is as follows: with the sliding rod eudiometer, throw 100 measures of the gas under trial above the surface of water that has been duly exposed to the atmosphere, and contained in an inverted bell, rather wide in proportion to its depth; one made of the belly of a glass retort or a cupping-glass answers very well. Then add 100 measures of the binoxide of nitrogen if the gas is suspected to be poor in oxygen, but 200 or more if the gas is richer, always observing to have the binoxide in excess. After the lapse of a minute the absorption is complete; measure the residue, and one fourth of the diminution gives the volume of oxygen: this method is analogous to that of Gay Lussac. Some idea of its correctness may be formed from the circumstance that, of 73 analyses of the air, the mean result of the amount of oxygen is 20·58 per cent. My measuring rod divides each volume into decimals by a vernier arrangement; but for most purposes of analysis this is unnecessary.

50. It results, from the observations which have been made on caoutchouc by Dr. Mitchell, that oxygen passes through it with much more facility than nitrogen. Atmospheric air is also reputed to be a mixture, and not a chemical compound; it was therefore an object to try whether pure oxygen might not be obtained by forcing air through such a membrane, filtering, or, in fact, straining it through a gum elastic bag. A thin piece of this substance was therefore tied tightly over a tube an inch in diameter and six inches long; the tube was then filled with mercury in such a manner that the great weight might not burst the caoutchouc; it was then inverted and exposed to the atmosphere. The membrane bulged into the tube in a deep hemispherical form; in about an hour its under surface was studded with bubbles of gas, and in the course of time several cubic inches passed. This, on analysis, by means of binoxide of nitrogen, was found not to differ sensibly from atmospheric air. A similar result was also obtained when a thin serous membrane, a piece of peritoneum stripped from the liver, was substituted for the gum elastic. No indications whatever could be obtained that atmospheric air was decomposed during the process. Nor is it difficult to understand and explain how this happens, when a foreign force, equivalent to a pressure of six inches of mercury, is brought to bear so advantageously on the action of a very thin membrane; for in the case of the gum elastic, the thickness could not be estimated at more than $\frac{1}{300}$th of an inch, and the serous membrane was so porous that it could not sustain so heavy a pressure without immediate leakage; the united gas, whatever it may be, is at once forced through, the barrier being unable to stop it. A case of the same kind is met with when porous charcoal is used: pressure forces a gas through it entirely unchanged; but if the effects of that pressure be avoided, chemical decompositions of a decisive character may ensue, as we shall shortly have occasion to see. To obtain these chemical effects, it is necessary that the barrier should not only have no pores of sensible size, but that no adventitious or foreign forces be brought to act on the passing gas; in proportion as these conditions are fulfilled, the success of the experiment is more perfect; and thus, as we shall proceed to point out, it is possible to strain the nitrogen out of atmospheric air, and procure by that means oxygen of greater or less purity.

51. The doctrine laid down in sections 47 and 48, of the condition of equilibrium of gases on each side of a membrane, being the foundation of an explanation of all the phenomena which have as yet been noticed, requires farther consideration and fuller proof. Some remarks have been offered on the incomplete results which are obtained by the use of barriers consisting of pores of large size, such as stucco plugs. It is said, however, that in the hands of Mr. Graham these have given some curious results respecting the rate of diffusion of gases; experiments at once satisfactory and singular.

52. The objections above mentioned have, however, appeared to me so weighty, that I have not made use of such barriers, but resorted to liquids, which, for closeness of texture, uniformity of composition, and, above all, on account of our accurate knowledge of their habitudes and structure, are much preferable. They, also, have given results as curious, but far more satisfactory; and though, in the management of them, something of that dexterity of manipulation is required which practice alone can confer, yet they are easy of repetition, never failing to give precise and comparable results. They also afford the means of prolonging or hastening the close of an experiment, which at times is invaluable; their action, too, is very uniform; for a film of water so thin as to be coloured acts as well as a mass several inches in depth, but the gases passing through it more rapidly, a state of equilibrium on both sides is obtained in a few minutes. The following facts will serve as an illustration: Into a tube *b* (*fig.* 10, *pl.* 1), which was conoidal at its upper end, a disk of paper, *a*, was fastened water-tight, and then upon that was poured distilled water till it was about $\frac{1}{8}$ inch deep; the tube was next filled at the pneumatic trough with hydrogen gas, which passed into the atmosphere through the paper roof, and the water reposing on it; but, though the tube was only $\frac{3}{8}$ inch in diameter, twenty-four hours elapsed before a column of hydrogen half an inch long had gone out, and in seven days only one inch more. A common glass tumbler was filled with hydrogen gas at the pneumatic trough, and by the side of it stood a small bottle, the height of which was about $1\frac{3}{8}$ inch, its diameter $1\frac{1}{4}$ inch, and the diameter of its neck $\frac{3}{8}$ of an inch. The atmospheric air in this bottle being of the same temperature as the hydrogen in the tumbler, a finger dipped in water rendered slightly viscid with soap was passed over the mouth of the bottle, so as to leave a thin film stretched there, the tumbler of hydrogen being then placed over it (*fig.* 12, *pl.* 1). In the course of two minutes, the film, instead of being horizontal, became convex, and continued to be so until it had swelled into a large spherical bubble, which capped the top of the bottle; in sixteen minutes this had increased so much in size and become so thin that it was of a dark metallic lustre, and it burst at last by swelling, so as to touch the bottom of the tumbler. During this experiment the barometer was at 28·8; thermometer at 68·75, Fah.

53. The rapidity of this action being proportional to the thinness of the film used as a boundary, it is obvious that the duration of an experiment may be managed by determining beforehand the thickness of the film through which the gases shall pass. If very thick, the time may be indefinitely long, and if very thin, indefinitely short. Nor need we be limited in reducing the thickness to the greatest extent, for it is found by experiment that, however thin the film may be, it still possesses cohesion enough, and its parts

are still so close, that anything like mechanical straining or leakage cannot take place through it. The first attempts to ascertain the laws of movement and equilibrium of gases passing through liquid films were made by stretching those films over the mouths of vials, as here described; subsequently, for several considerations, this arrangement was given up: the horizontal film is at first too thick, it exposes too small a surface to the atmosphere to which it is subjected, and it is not until towards the close of the experiment that the action becomes at all rapid. Bubbles of water made sufficiently adhesive by a little soap were, therefore, substituted. One of these filled with any gas, and immersed in an atmosphere of another gas, at once exposes a large surface, and, by swelling or collapsing, allows a free action. There are, however, three circumstances which tend to destroy such bubbles, and against these provision should be carefully made. Mechanical agitations of the surrounding air may be met by covering the whole arrangement with a glass bell. Evaporation from the surface of the bubble, which reduces its substance unduly, may be avoided by keeping all the gases under trial in jars over water, until they are loaded with moisture, and thoroughly wetting the inside of the covering-bell; but it is not so easy to prevent that slow motion of the parts of the bubble, which, in virtue of the earth's attraction, tends gradually to bring them to the lowest part, while the walls of it become too thin to bear the weight, and are liable to burst by the expansion of the gases accumulating within.

54. After a number of trials, the following has been found to be the most suitable arrangement for prosecuting these inquiries; it is simple, not easily deranged, and allows of sufficient latitude and change to suit other experiments. In it a soap bubble may be preserved with certainty, for a time considerably exceeding an hour, and sometimes much longer. As here described, it was used to illustrate the relative passages of hydrogen, oxygen, and nitrogen through a watery film into atmospheric air. It is represented in section; A A (*fig.* 11, *pl.* 1) is a small tin saucer, about three inches in diameter and half an inch deep; into it water can be poured, and it also serves as a platform to support a large cupping-glass, *b*. Through the centre of this tin saucer, at *c*, passes a glass pipe, *f*, $\frac{1}{8}$ inch in diameter, the upper extremity of which is cemented into a hole of the same size in a round, thin piece of copper, *d*, which is about half an inch in diameter, the other end of the pipe opening into another cupping-glass, *k*, through a perforation in its top, the communication being capable of being cut off by means of a cock, *g*; the lower cupping-glass serves as a support to the whole arrangement when placed upon the shelf of the pneumatic trough. This apparatus is used as follows: The upper cupping-glass being taken off the platform, is filled with any gas under trial, as oxygen, and placed aside on the shelf. The lower cupping-glass is then filled with water by depressing it in the trough, and the cock being closed, five hundred measures of hydrogen, for instance, are thrown into it. After seeing that the copper plate, *d*, is free from moisture, a drop of water, rendered slightly viscid by soap, is placed upon it exactly over where the orifice of the pipe, *f*, opens. The upper glass, containing the oxygen, is now placed upon the little tin saucer platform, as in the figure. The lower glass is next depressed in the trough, and as soon as the cock is opened, a bubble of hydrogen containing five hundred measures expands, the spare oxygen escaping from the

edge of the upper glass through the water in the saucer; the cock is next closed, and the apparatus placed on the trough shelf as long as the operator desires the experiment to continue. Keeping that position when the cock is once more open, the gas passes into the lower glass until the bubble is entirely collapsed, when the cock is again closed, the contents of the bubble being now ready for measurement or analysis. As the gas passes from the bubble into the lower jar, the water rises from the tin saucer into the cupping-glass above, confining the gas that was outside of the bubble; this, by the common mode of manipulation, is to be transferred from the tin platform to the shelf of the trough for inspection.

55. By this apparatus it was found that one thousand measures of atmospheric air, exposed in a bubble to atmospheric air, in five successive experiments, underwent no change either in volume or composition. The duration of the trials was severally ten, fifteen, twenty, thirty, and sixty minutes, and the uniform result, when drawn back into the under cupping-glass, was one thousand measures exactly, the composition of which was the same as atmospheric air.

56. The thermometer stood at 54° Fah. One thousand measures of hydrogen in the watery film were subjected to atmospheric air in the upper bell; in five minutes there remained only four hundred and seventy-two. In the second trial, one thousand measures in twenty minutes became-four hundred and thirty-two; and in a third, when the same quantity of gas was confined half an hour, the residue was four hundred and eighty measures.

57. A reverse action ensues when nitrogen is substituted for hydrogen: the bubble swells instead of diminishing, and the resulting gas measures more. It is to be remarked, that after the first five minutes, provided the bubble has been sufficiently thin, there appears to be little or no change in the volume of gas, and in a great many experiments it was found that motion had ceased when the bubble had increased somewhere between $7\frac{1}{2}$ and 10 per cent. The thermometer standing at 55° Fah., one hundred measures of nitrogen in half an hour became one hundred and seven and a half. In another trial, two hundred measures in the same time became two hundred and fifteen. Again, two hundred in fifteen minutes became two hundred and sixteen. The greatest variation from this was in one case, when, after an exposure of five hundred measures for five minutes, the bubble was found to contain five hundred and forty-five measures, or an increase of 9 per cent.

58. Oxygen gas exposed in like manner to atmospheric air, decreased in bulk; thus, two hundred and fifty measures in ten minutes became one hundred and fifty-three, and the like quantity in fifteen minutes diminished to one hundred and forty-four, which amply proves that the passage of oxygen takes place through water more rapidly than nitrogen. And upon this fundamental principle, chemical decompositions can be effected; as in the last section, where we have a bubble of nitrogen gas exposed to the atmosphere, the nitrogen outside parts with its oxygen, and passing through the barrier, unites with the oxygen within.

59. Having thus recognised a variation in the rate of passage of gases through thin films, it becomes a point of investigation to ascertain how long these motions may be

maintained, and under what circumstances a state of equilibrium will ensue. I have already stated that the condition of rest was simply an identity of composition of the media on both sides the membrane, a law which is rigidly observed by all gases that have yet been tried. Four hundred measures of nitrogen gas procured by phosphorus, but which, by standing over water, were found to have gained 3½ per cent. of oxygen, were exposed to atmospheric air, in the apparatus above described, for thirty minutes: at the end of that time, there were found four hundred and thirty-two measures in the bubble, of which 15½ per cent. were oxygen. Outside the bubble were ten hundred and seventy measures, which also contained 15½ per cent. of oxygen; thermometer 57° Fah.

60. Two hundred measures of nitrogen, containing the impurity as above, were exposed for thirty minutes in an atmosphere of impure oxygen, which contained nitrogen and carbonic acid, to the amount of 13½ per cent.: at the end of that time, three hundred and sixty-one and a fourth measures were found in the bubble, of which 62 per cent. were oxygen; and eleven hundred and forty-four and a half measures were found outside, 62½ per cent. of which were oxygen; thermometer 55° Fah.

61. Two hundred measures of oxygen were exposed to an atmosphere of hydrogen for fifteen minutes, at a temperature of 66° Fah.: at the end of that time, two hundred and seven and three fourths were found in the bubble, containing 16¾ per cent. of oxygen; and twelve hundred and seventy-three outside, which also contained 16¾ per cent. of oxygen.

62. The slower passing gases being thus found to obey a very simple law of equilibrium, attempts were made to ascertain whether such as carbonic acid, which are very absorbable by water, followed the same law; but, after many trials, no certain result could be obtained, so rapid was the action. Five hundred measures thus confined passed out immediately, the bubble collapsing almost as fast as it had been expanded: a tube was therefore prepared, which had a roof of water at one extremity, about half an inch thick and two inches in diameter; beneath this roof five thousand measures of carbonic acid gas were placed, and the arrangement exposed to the atmosphere. In forty-eight hours, analysis showed that a trace of carbonic acid still existed in the tube, which, when washed off, about two hundred measures of unabsorbable gas remained, consisting of 20·5 oxygen, 79·5 nitrogen; and therefore atmospheric air. This experiment would thus warrant the conclusion that gases of any kind will pass a barrier, subject to the same regulations as those that are less absorbable; had it been allowed to continue for a sufficient length of time, there can be no doubt that all the carbonic acid gas present would have escaped into the atmosphere, and atmospheric air alone been present on both sides of the barrier.

63. Hence, the condition under which motion ceases through a barrier is identity of chemical composition on both its sides. As gases, however, pass with different degrees of velocity through the same liquid, results seemingly anomalous may be obtained, and chemical decomposition may ensue; if water recently boiled be exposed to the atmosphere, it will be found in a few hours to have abstracted oxygen and nitrogen gases, not in the same proportion, however, that exists in the circumambient air, for the gas found in water contains ⅓ instead of ⅕ of oxygen; perhaps in the course of time that

richer gas would escape, and its place be taken by common air. We therefore consider this a case in which equilibrium has not ensued, progress only being made towards it, the decomposition and apparent anomaly being only the result of a more ready solubility and rapid passage of one gas. By taking advantage of this, it is possible to obtain from the atmosphere oxygen of some purity. If a volume of atmospheric air be agitated with boiled water in a close vessel, it will be found that a rapid absorption of its oxygen ensues, while but little nitrogen is imprisoned among the pores of the liquid. This gas, by the action of heat, may be driven off from the water, and being subjected to another washing, may be rendered still more pure; by successively washing and rejecting the nitrogen left, a gas so rich in oxygen may be procured as to be equal to some that is obtained by other processes, as by the action of sulphuric acid on peroxide of manganese.

CHAPTER IV.

ON INTERSTITIAL MOVEMENTS, *being a Continuation of the preceding Chapter.*

(From the American Journal of the Medical Sciences for May, 1836.)

CONTENTS: *Diffusion takes place between the Particles of Heterogeneous Bodies.—Differs from Chemical Attraction.—Action of Binary Arrangements.—Action of Ternary Arrangements.—Decompositions by Binary Arrangements.—Decompositions by Ternary Arrangements.*

64. IF we could place a known volume of vapour in the centre of an extensive void, where no disturbance from without could solicit its particles to move in one direction rather than another, it is to be supposed that, conformably to certain laws that are known to obtain and operate on bodies of an aerial constitution, movement would ensue. To an assignable limit the vapour would expand, by a species of repulsion of its own particles. In the immense vacuum in which the solar system moves, there are orbs that seem to fulfil this condition; these, though they wander through very large paths, and are disturbed by the reaction of bodies they move past, sufficiently approximate the circumstances here laid down to show that there is an extent beyond which bodies so constituted are not disposed to expand. Astronomical observations also show that gases such as our own atmosphere is composed of do not by their expansion trespass beyond a given point into a void, for then the laws upon which they are formed react, as the firmest barrier from without would do, to prevent their farther expansion.

65. An orb so constituted could not, in any length of time, undergo any change of composition, structure, or figure; for, so soon as the first motion which decided its equilibrium was over, that equilibrium would remain undisturbed, unless forces from without were brought to bear upon it. In a void such as we are here supposing, apart from any such derangement, equally a void as to force as well as to matter, the vaporous mass could not be subject to any contingency.

66. Let us extend our supposition by placing another volume in presence of the former, differing from it in chemical composition alone. That difference would determine certain motions of penetration, in addition to those resulting from mere mechanical action. Not only would the united mass move so as to assume a mechanical equilibrium, but its constituent parts would also move, so as to establish a chemical equilibrium at the same time. Wherever an atom of one of the vapours existed, there would be found one of the other also. To bring about this result, a mutual penetration of parts is demanded, a transit of the constituents of one vapour among those of the other. The motions that effect this arrangement take place without any resistance, just in the way that the light of a distant star comes into our system, undeterred by the rays of our sun, and moves freely in every direction; his beams also move in the vacuum, intersecting the paths of other luminous bodies without any hinderance or shock.

67. In this state of extension, when the component atoms of a gas or vapour are supposed to be stretched to their utmost limit, which we are prone to imagine can only be done by an increase of the distance usually existing between an atom and its neighbours, it is not difficult to suppose that these different motions can go on, and that a foreign atom may insinuate itself in the interstices between others. But our ideas of space and size being only relative, and as we know nothing of the dimensions of an ultimate atom, nor of the interval that parts it from those around it, it is plain we could not, without actual experiment, determine when a body had arrived at that state of condensation, or when its particles had become so closely approximated to each other as to refuse the admission of foreign atoms between them.

68. A mass of any kind in a vacuum, and undisturbed, moves, therefore, only in that manner which the laws of dynamics indicate. Motions of another kind, however, are induced when the vacuum is changed for a substance; a kind of penetration, permeation, or absorption is the result; nor do the mechanical conditions of bodies appear to have any effect: with some of these phenomena we are familiar. A gas, a liquid, or a solid may indiscriminately pass by solution into the pores of water without any reference to their aggregation. A variety of words have been used to express this action: solution, endosmosis, permeation, &c.; but, parting from the simplest experimental condition, we shall have occasion to see that all these refer to varieties of one phenomenon only.

69. If a solitary body has thus no opportunity of exhibiting the conditions of its own arrangement as to structure or the forces that inhabit the interstices of its atoms, it is very different with a binary arrangement. Chemists are familiar with the phenomena exhibited when gases, solids, or liquids are exposed to each other under those circumstances where no direct change of composition ensues. Thus, if a cubic inch of carbonic acid gas be exposed to a cubic inch of water, the gas in a short time passes into the liquid mass, or is absorbed by it, with a certain degree of force and to a certain amount. Also, aqueous gas rises from the water, and diffuses itself into the unabsorbed remainder of the carbonic acid. After a sufficient time, no part of the carbonic acid will be found destitute of aqueous gas, nor will any part of the water be without its equivalent of carbonic acid. The simplest example of these combinations is furnished by the solution of saline bodies in water, where there is no change of chemical composition, but merely

a detachment of the solid crystalline particles from the mass of the dissolving substance; these pass among the interstices of the liquid, and remain there, unaffected by gravity, being equally and uniformly diffused. Of the powers by which this is brought about we are not well informed, but no fact in science is better ascertained than this uniform and equable diffusion. If, by affinity, we mean a power that causes substances to unite with an interchange of elements, or one which is only exerted to bring about an alteration of composition, such a force is obviously insufficient to give rise to these effects.

70. That one particle has the power of attaching itself to another of a dissimilar kind, without anything like change of composition, numerous facts demonstrate. The delicate dyes that adhere to cloth-fibre offer an example; they cannot be supposed to be attached by any force affecting either their composition or structure, since the successful operations of the artist proceed upon the supposition that the tint shall be unimpaired, and the strength and organization of the fibre which is dyed shall remain untouched. Now in those cases where we know that the dyeing material acts chemically on the fibre, is there not abundant proof that the elementary changes affect the uniting bodies? Is not the hue of the dye changed, and does not the fabric become rotten? Other facts also show that these adhesions, without chemical change, are possible; the foil on the back of a mirror is not retained by the exercise of any force which has brought about a change in its composition. When the dye is washed off, or the foil scraped away, the cloth-fibre and the looking-glass are both found in their original integrity of structure.

71. The cases here cited furnish examples of one solid uniting to another in a manner that involves something different from the action of chemical affinity. There is a whole range or class of similar combinations: a solid may unite thus with a liquid, as sugar and water; a liquid with a liquid, as alcohol and water; a liquid with a gas, as carbonic acid and water; or a gas with a gas, as oxygen and nitrogen. All these are cases where there is no interchange of chemical elements, and which we cannot, therefore, suppose to ensue in virtue of chemical force.

72. Although these actions are the result of a kind of adhesion of particle to particle, and might, therefore, be supposed to take place in an indiscriminate or irregular manner, there are some remarkable circumstances attending them which go to show the contrary; thus, water will dissolve a certain quantity of sulphuric ether, and no more; it will take up its own volume of carbonic acid, and no more; it will hold in solution of bisulphate of potash, sulphate of ammonia, protosulphate of iron, bicarbonate of potash, chromate of potash, muriate of strontian, &c., half its weight, at 60 F. At the same temperature it dissolves its own weight of sulphate of magnesia, and this comparison might be extended much farther. The same kind of predilection for definite quantities obtains also in gases, as is the case with atmospheric air, where the proportions of oxygen dissolved in nitrogen are as one to four, nearly.

73. All these things go to prove that the passage of the particles of one body among the particles of another proceeds upon certain and definite laws. Whether the residence of saline atoms among the interstices of a liquid is a phenomenon of the same sort as the adherence of dye to a fibre, it is not material to inquire. We know, by

experiment, that a solitary gas has a tendency to expand itself to a certain extent, but not farther; and we are equally assured that bodies, whether of the same or of different kinds, have an inclination to penetrate into each other. Where there is an apparent indisposition to do this, we are not without plausible reasons for supposing it to be through the intervention of disturbing causes. If oil and water do not commingle, it is a result determined by the action of their cohesion, as compared with the force of attraction between them. An interesting example of this nature is afforded by the action of mercury on glass: under ordinary circumstances, they show no disposition to unite, not even so much as water and oil; but, by a suitable application of heat, the cohesion of the mercury may be so lessened, and its force of attraction for glass at the same time so exalted, that it can be brought to *wet* it; an experiment first successfully performed by Laplace.

74. This nisus, or endeavour of one body to diffuse itself into the interstices of another, has, under a variety of forms, been long recognised. The solution of salts, the absorption of gas by liquids, the passage of liquids through crystals, the permeation of porous textures, the diffusion of gases, the languid movement occurring in solids, were known long ago. Of late years, some extension of these facts has been obtained, and the new phenomenon, though explicable on the same principles, is dignified by the title ENDOSMOSIS (37).

75. For the explanation of the whole of this most interesting series of results, one postulate alone is demanded—that *all bodies have a tendency to diffuse themselves into the interstices of all others, with more or less intensity.* Nor is it difficult to admit this principle in its fullest extent, when we consider the numerous examples philosophy affords of it. All kinds of chemical absorptions and solutions are cases of it. The disturbing causes which sometimes change, or even entirely hinder these actions, we shall consider hereafter.

76. BINARY ARRANGEMENTS, or those in which two bodies are engaged, whether solid, liquid, or gaseous, exhibit some circumstances which it is here necessary to point out. Let us suppose the couple under consideration to be oxygen gas exposed to an equal volume of water. No remarkable phenomena attend the passage of the gas into the liquid; there is no rise of temperature, and the whole amount absorbed is greatly less than the bulk of the water. If another gas be substituted, as carbonic acid, though much more soluble, there is still no indication of change of temperature, but ammonia and muriatic acid condensing to a much greater amount, disengage heat. Another couple might be assumed, as charcoal or porous masses, with oxygen or other gases, and similar indications be obtained.

77. If, after a liquid has absorbed as much of any given gas as it is capable, we remove the remnant of unabsorbed gas, and in its place substitute some other of a different kind, complex reaction ensues. The gas, already absorbed by the water, has its condition of equilibrium disturbed, and, in conformity with the general principle (75), it has a tendency to diffuse itself out of the water into the newly-introduced gas. This, in its turn, has also a tendency to pass into the water. Thus, if over a volume of water, impregnated with carbonic acid, and confined in a jar over mercury, we place a vol-

ume of oxygen, equilibrium would not be obtained until a certain amount of carbonic acid was found in the gas, and a certain amount of oxygen in the water. And the same would hold in the case of any other gases or any other liquid. In the course of experiment, examples of this case are often met with. The water commonly used in pneumatic troughs contains both oxygen and nitrogen. If into a jar containing such water we pass pure nitrogen, in the course of a few minutes oxygen will leave the water to diffuse itself into the nitrogen. Had we thrown in pure oxygen, nitrogen, on the contrary, would have deserted the water and mingled with the oxygen gas. In gaseous analysis, this action, which obtains to a greater or less extent with every gas, often gives rise to much perplexity.

78. Ternary Arrangements.—It is plain that the conditions of the action considered in the last paragraph may be obtained *at once* by suitable arrangements; and, as it is important that these should be well understood, I shall dwell upon them minutely.

79. In paragraph (77), we considered the reaction ensuing, first, of a single couple or binary arrangement, and then the disturbance effected by the introduction of another element. Could we, then, *at once* have exposed the volume of water by one surface to oxygen gas, and by another to carbonic acid, the changes that were consecutive would have been simultaneous. Let *a* (*fig.* 13, *pl.* 1) be a sheet of water, on which, at its upper surface, a volume, *b*, of carbonic acid reposes, and beneath its under surface, *c*, a volume of oxygen; both gases pass *at once* through the water, in opposite directions, into each other. It is evident that the thinner we make the barrier of water, the more rapidly will equilibrium be obtained. This I have accomplished in the following manner, by using mere liquid films, and for that purpose have taken advantage of soap and other bubbles. A glass tube ⅛ inch in the bore, and seven or eight inches long, is to be drawn out at one extremity to a capillary termination, and when the bubble is to be blown, the other end is dipped into a solution of soap. The tube having been previously passed through a cork, as in *fig.* 14, *pl.* 1, is now to be introduced into a clear vial or bell glass, the neck of which the cork fits loosely; on blowing at the capillary termination, the bubble slowly expands in the vial, where it is protected from access of air. To measure its diameter, I take a strip of white pasteboard, and divide it into inches and decimals, placing it in such a position before the vial that it crosses the bubble diametrically; then with a small telescope that magnifies twelve or twenty times, and at the distance of about eight feet, I observe the bubble much magnified, the micrometrical pasteboard apparently passing through its very substance, as is shown in the figure.

80. Through a soap bubble 1·53 inch in diameter, the substance of which, previous to expansion, was contained in a cylinder ⅛ inch in diameter and ¼ in height, ammonia, either pure or diluted with atmospheric air, passes instantaneously when air from the lungs is on the other side. Into the bottle in which the bubble is to be blown a little strong solution of ammonia is to be poured; the bubble is then expanded; at a particular point it becomes dyed with the richest hues, and that moment the phenomenon of endosmosis is complete: care must be had to suffer no moisture from the mouth to close the capillary termination of the glass tube; and now a rod, *a*, dipped in muriatic acid, is to be brought over the opening; as the bubble is collapsing by the attraction of

its own parts, dense fumes of muriate of ammonia make their appearance, which continue until the substance of the bubble has entirely returned into the tube. The extraordinary rapidity of this action is remarkable. The bubble is scarcely blown before it is full of ammonia; and it is not less interesting to observe how the colours play with change of atmosphere. A little cylinder expanded to the size of a pea, which, in common air, is opaque white, and which would not be coloured until expanded to six or eight times that diameter, becomes deeply tinged as soon as it is penetrated by ammonia. If restored to the free atmosphere it loses all its beauty, and these alterations may be kept up at pleasure by merely changing it from one medium to another.

81. When, for the purpose of experiment, it is desirable to have a permanent bubble, a small column of moisture from the tongue must be allowed to close the capillary termination of the tube.

82. In the same manner, hydrosulphate of ammonia is found to pass with instantaneous rapidity through the film, and may be detected by a paper dipped in acetate of lead. The colours in this case become very quickly stable, as with ammonia, and do not produce that iridescent play which the passage of certain other substances affords. It is, however, essential to the success of these experiments that the substances about to be passed through the film shall not have any chemical action upon it. Thus, it is not possible to use muriatic acid, which decomposes soap, but there is no difficulty in the management of such as oxygen, hydrogen, nitrogen, &c. The passage of hydrogen through these films is exemplified in the following table:

Diameter of a bubble of hydrogen gas exposed to atmospheric air.

Exp.	When Blown.	In two Minutes.	*
1	·460	·430	·430
2	·415	·390	390
3	·425	·400	·400

Diameter of a bubble of atmospheric air exposed to hydrogen gas.

Exp.	When Blown.	In two Minutes.	*
1	·470	·485	·495
2	·360	·375	·380
3	·420	·435	·445

83. The third column, marked *, in these tables was taken when the black spot on the top of the bubble was about half an inch in diameter; for, as the coloured rings were the same in each experiment, and the surface incapable of reflecting light of equal extent, it is to be presumed that the measures were obtained under like circumstances, as far as the thickness of the film was concerned. In all cases the bubbles were blown by pressure on a gum elastic bag. This method of measuring the expansion, though suitable for general purposes, cannot, however, be extensively relied on, owing to thermal disturbance and the earth's action changing the figure from a true sphere to a prolate spheroid.

84. It is interesting to remark with what extraordinary rapidity these permeations take place. If we expand a small bubble in a vessel of ammonia, hydrogen, sulphuretted hydrogen, &c., by means of the mouth, and, without removing the lips from the capillary opening of the tube, inhale *immediately* the contents of the bubble, the gaseous matter will impress the organs of taste with a very distinct savour, peculiar to the gas on which the experiment is tried. There is a class of vapours which appears to possess little or no affinity for water, such as ether and the essential oils; these, however, percolate through tissues of water with rapidity. On covering the bottom of a vial with oil of pep-

permint, and then expanding a bubble, the taste of the essential oil will be perceived when a portion of the air is drawn back out of the bubble into the mouth. With other oils, as cajeput, and with ethers, the effect is the same; and it is to be observed, that during the transit they work the surface of the bubble into a kind of microscopic waves, and produce an iridescent play of colours.

85. To obviate any exception that might be taken to the use of soapy matter in these films, or to their excessive thinness, I have employed the arrangement of section 52, which establishes the same truths.

86. Into such a tube I threw 200 measures of hydrogen gas, and the same quantity into one the upper extremity of which was hermetically sealed, by way of affording a comparison. In the former, the thickness of the roof of water was about ½ inch, and in 24 hours the level of the water in it rose half an inch; while in the latter it remained undisturbed, thus incontestably proving that hydrogen gas passes with great freedom through masses of water. Nor is this permeability confined to that liquid alone: a tube which was thus covered with a layer of lamp oil, in five days raised the level of the water in it more than two inches; and one the roof of which was of copaiba balsam, threw out all the gas to within ½ of an inch of its top; while a tube of the same size, but sealed at the other end, that stood by them, kept its level.

87. It appears to me the reason that we have not hitherto understood the phenomena of endosmosis, or the action of these ternary arrangements, as I have called them, has arisen chiefly from the employment of substances as barriers, which were possessed of pores of sensible size. A moment's consideration will place this in its true light: suppose two gases were kept apart by the intervention of a plug of charcoal, in their diffusion into each other, not only would those portions pass the barrier which were brought along by a direct action, but a much larger quantity would slip through by mere leakage among the pores (36). Bladder, tissues, stucco plugs, &c., which we know to possess pores of sensible size, are open to this objection; but the case is very different with liquids, which, from their uniform condition and the close proximity of their atoms, admit of no such action. A mass of stucco a foot thick would be subject to this kind of mechanical derangement, but a sheet of water reduced to that excessive degree of thinness that it is invisible, allows no gas to go through it by leakage, but all passes by absorption.

88. The original experiment of Dutrochet on endosmosis, and those of Dr. Mitchell, were examples of this class of ternary arrangements. In these cases, membranes or gum elastic were tied over the mouths of vessels, and the result was shown by the swelling or sinking of the barrier. These can be repeated in a more satisfactory manner with liquids (52).

89. In this experiment we also recognise an identity of results with those which have heretofore excited so much attention, under the title Endosmose; but understanding in this case, as we do, the conditions under which the result is obtained, there is no difficulty in extending the explanation of one experiment to the other. Endosmose is only a complex case of simple absorption. The mechanical results here obtained, the swelling or sinking of the barrier, depend on the more rapid absorption of one gas by that

barrier. The condition under which we obtain the mechanical result will, by being duly varied, also furnish chemical results, an investigation of which forms our next object.

90. Chemical Decomposition; *and, first, by* binary arrangements.—A solitary arrangement of any kind, whether it be of a simple or of a compound nature, has no power of change in itself; but it is conceivable that one of the latter kind—compound—on forming part of a binary arrangement, may be differently affected; as an illustration, let us take atmospheric air and water, appropriately situated, to form such an arrangement (63). In this case, were the nitrogen and oxygen equally absorbable by the liquid, no remarkable result would ensue; but such is not the fact; the oxygen gas passes much more quickly into the water than the nitrogen, and decomposition takes place, an excess of oxygen being in the liquid, and an excess of nitrogen being left. We should, therefore, expect that rain, and dew, and springs, and rivers, which have been exposed in a very divided state to the air, ought to contain a gas richer in oxygen than that of the atmosphere; and such, in fact, is the case, the atmosphere containing one volume of oxygen and four of nitrogen, the gas of water containing one of oxygen and two of nitrogen, as we shall shortly find.

91. Instead of a gas and a liquid to form these binary arrangements, a solid and a gas may be used. Into 500 measures of atmospheric air, a piece of charcoal, that had been made red hot and quenched under mercury, was placed. The volume of the air experienced a rapid diminution, and after the absorption had gone on for several hours, there remained 205 measures, 100 of which contained only eight of oxygen. The charcoal was now introduced into water over mercury, and commenced very actively evolving gas, which contained only 3·75 per cent. of oxygen, and the last portions of it that were given off, only 2·8. Solution of lime was not capable of detecting the presence of carbonic acid in the water.

92. In the place of charcoal, other porous solids might be substituted; into a jar, *a*, which contained atmospheric air, there was introduced a piece of red-hot pumice stone; into *b*, a piece of clay that had been made red hot; and into *c*, a piece of charcoal quenched under water. Absorption took place in them all, and in a quarter of an hour *a* was found to contain 19 per cent. of oxygen, shortly after *b* was found to contain 19 per cent. of oxygen, and *c*, in half an hour, only 18 per cent. Also, in five hours, *c* only contained 17·25 of oxygen, and in seven days, only seven per cent.; but, at the same time, *a* and *b* contained 14·50 per cent. Four days after, *c* contained only five per cent.

93. By long boiling, I extricated all the air possible by such a process, from a quantity of water, and pouring it into a glass cup, left it exposed to the atmosphere for some days; at the end of that period the water was again boiled in a close vessel, and the gaseous matter it had absorbed submitted to analysis. After the carbonic acid had been carefully washed off, its amount being about 29 per cent., it was found that the residue contained 32½ per cent. of oxygen gas. It is a singular fact, that an aqueous mass, in thus decomposing atmospheric air, appears to follow a very simple law; pure spring water and distilled water, after a competent exposure to the atmosphere, are found to contain a gas whose elements are not in the proportion of one to four, as in the case with the atmosphere, but in the proportion of one to two. In several analyses of the air, extrica-

ted by boiling from the water of a spring, which flows from a sandy valley, and also from the dews which fall on a neighbouring hill, but too remote to be affected by the exhalations of dwellings, I found the proportion, when care was taken in the analysis, to be uniformly $33\frac{1}{3}$ per cent., or as 1 to 2 by volume. This gas, thus extricated, is isomeric with protoxide of nitrogen, with the particular exception that, in the protoxide, the two volumes of nitrogen are compressed into half their bulk.

94. In a quart jar, which was filled with spring water and inverted into a tin capsule, I collected all the aeriform matter that could be disengaged from the water by means of a fire placed beneath the jar and its tin. This gas, from many prior trials, I knew to contain $33\frac{1}{3}$ per cent. of oxygen. When all the gas was collected that could be extracted, at a temperature long continued close upon the boiling point, the arrangement was suffered to cool, and kept undisturbed for four days; at the close of that time, considerably more than three fourths of the gas disengaged was reabsorbed; the residue, on analysis, contained 5·25 per cent. of oxygen only. A portion of the water in the jar was now submitted to a boiling temperature in a small close vessel, and the gas collected was analyzed. It contained, instead of $33\frac{1}{3}$, rather more than 47 per cent. of oxygen. There cannot, therefore, be any doubt that oxygen may be obtained from the atmosphere, in a pure and undiluted state, by the action of a tissue, or a binary, and also a ternary arrangement.

95. Decompositions by Ternary Arrangements.—After this consideration of the case, in which two elements are employed, we are prepared to understand how ternary arrangements effect decompositions. Let *b* (*fig.* 13, *pl.* 1) be a compound gas, which is placed above a barrier, *a*, of such a nature that one of the elements of *b* shall pass more rapidly through it, or, in other words, be more readily absorbed by it than the other. Also, let the other substance, *c*, which is on the opposite side of the barrier, be of a kind capable of removing the quicker passing element of *b* from the under surface of *a*, as fast as it arrives there. It is immaterial how this removal be accomplished, whether by chemically uniting with it, or by mechanical action; the quick passing element, finding at its approach to the under surface of the barrier a ready exit, continually passes off, and its place is supplied by fresh portions from above, so that, in the lapse of time, only the less absorbable element will be found in *b*.

96. The general conditions, therefore, of chemical decomposition by ternary arrangements are, that one element of the compound to be decomposed shall pass more easily through the barrier or bounding tissue than the others, and, on its arrival at the opposite side of the barrier, it shall be rapidly removed.

97. Reasoning upon this principle, I succeeded, nearly two years ago, in effecting decompositions in this manner, which have some important physiological applications. Having taken a tube, one of the ends of which was expanded into a trumpet-shape, and closed with a thin serous membrane—peritoneum stripped from the liver—which was tightly tied on with a waxed thread, while it was wet, I poured through the orifice, which was open, a strong but clear solution of litmus in water. The tube thus situated was placed in a wine-glass containing strong alcohol, and the level of the liquid, inside and outside, made to coincide. The conditions for decomposition were thus fulfill-

ed; the water could find a ready passage through the serous membrane, but the colouring matter could not. Now, on arriving at the under side of the membrane, the water either was removed by uniting chemically with the alcohol, or by sinking mechanically through it to the bottom of the glass. Complete decomposition was effected, all the colouring matter being retained above the membrane, and, on placing a candle on one side of the glass and the eye on the other, dense striæ of colourless water were seen passing through the alcohol, but not a particle of the litmus escaped.

98. Under this condition, those experiments which have been instituted to demonstrate the passage of colouring matter through the lacteals have been made. The lacteals do not open into an intestine with patulous mouths, but their lining membrane of serous tissue ends bluntly in a kind of cul-de-sac. Through such a membrane, litmus, indigo, &c., cannot penetrate, though water may find a ready passage. Hence, because we cannot colour the chyle by an injection of litmus or indigo water, it is not to be inferred that no medicine can pass from an intestine into the lymphatic system; the experiment now detailed goes to prove directly the reverse, and furnishes us with an explanation of the uniformity of colour of the fluid in the chyliferous vessels.

99. An important circumstance in gaseous analysis may here be noticed. If a tissue, in the act of transmitting gas, or ready to do so, be placed in contact with another gas of a different nature, disturbance immediately ensues. A cubic inch of nitrogen gas, made with phosphorus, but which was found to be contaminated with $4\frac{1}{2}$ per cent. of oxygen, was agitated briskly in a vial containing about an ounce of spring water, such as has been mentioned to contain a gas $\frac{1}{3}$ oxygen. In one minute the nitrogen gained one per cent. by the agitation. The same quantity of nitrogen gas, agitated in a pint of water, gained no less than 11 per cent. of oxygen, which it had taken from the rich gas of the water. Nor is agitation or mechanical violence necessary to produce this important result. Into a bell filled with water, and inverted into another vessel, so as not to touch it in any point, I threw 100 measures of a gas, 85 of which were oxygen. After four weeks an analysis was made, and the gas in the bell found to contain only 72 per cent. of oxygen, the remainder being nitrogen. In this way, too, in the lapse of time, from an inverted vessel, partially filled with atmospheric air, the oxygen will escape into the water, and thence into the atmosphere; and I have twice known this event to take place, so that the residue did not contain more than three or four per cent. of oxygen. In many of the most delicate researches of chemistry, we have this disturbing cause in operation. Water is universally employed in our laboratories as a means of confining gases; it enters largely into our processes of pneumatic manipulation; and though we have hitherto neglected its action, it silently disturbs all our results. An air-bell cannot pass to the top of a jar without instant contamination: during its residence there it is subject to a continued succession of changes—at no two moments is it the same in composition—a perfect freedom of communication existing between it and the atmosphere.

100. As an instrument of rigid analysis, the pneumatic apparatus, so arranged, requires to be used with circumspection. It is impossible to keep oxygen, nitrogen, or any other gas in its original purity, if confined by water. This fluid, which, when reduced to a

thin, imperceptible film, is instantaneously permeated by almost every substance, undergoes the like action in course of time, even in deep masses. Gases are absorbed by it, and thrown off by it in its purest state; how much more complicated, then, must its action be in that impure condition in which it is commonly used. Connected with this point there is another: if a series of bells stand on a pneumatic trough, each will affect all the others, communicating a part of its contents, and receiving from them in return. A jar, containing binoxide of nitrogen, standing by the side of one containing common air, seriously affects it. I have noticed two common tumblers, filled with these gases, and so placed, communicate with each other so freely, that in 17 hours the tumbler originally filled with atmospheric air contained only $9\frac{3}{4}$ per cent. of oxygen. The habit of collecting gases at the same trough, that is destined to preserve others, is very exceptionable; we place the disturbing agency in circumstances the most favourable for its action. All operations of washing are liable to the same strictures.

101. We have assumed it as a law of nature, that any substance, when placed in contact with another, has a tendency to diffuse into it.

102. It is to be remarked in reference to this, that no hypothetical cause is assumed; it is merely taken as one of those ultimate facts which the progress of knowledge has not explained. We do not consider whether it involves the position that two bodies can exist in one place at one time, nor do we deny the impenetrability of matter. But it is required of us by a crowd of facts to admit this law, as the only legitimate position on which they can be explained. We know nothing of the size, or figure, or condition of the ultimate atoms of bodies; there are, indeed, some circumstances which would lead us to suppose that, even in the densest structures, each particle is at an immense distance from those that are next around it, in comparison with its own diameter. In those interstices which must necessarily exist, these phenomena of absorption may take place in accordance with laws which obtain among the molecules of bodies. In the same way that a comet comes down from the regions of space and traverses a planetary system, receiving impressions greater or less from each star that it passes, and emerges back again untouched and unimpaired; so a gaseous particle may pass through the system of atoms that constitute a solid mass, and moving therein unimpeded and without contact with any of them, may emerge without change of physical condition, or only a mark that its motion has been subject to those laws which obtain in the system through which it has gone.

103. All these observations go to establish the point, that pores of a sensible size have nothing to do with endosmosis—that it is a phenomenon depending simply on absorption. No one would aver that water possessed any apertures, or vessels, or tubular arrangement.

104. The experiment of (80) does not alone prove that endosmosis takes place through liquids and tissues the pores of which have no sensible size; it has a much more interesting application. Physiologists know that the primitive form of all organic bodies is an imperforate vesicle or globule, having the power of absorbing those substances which are around it, and decomposing them. The ultimate vesicle yields to analysis carbon, the elements of water, and a few salts. It is a centre of vital activity, a

laboratory assimilating things for its own substance. The simplest plants, confervæ, tremellæ, and the simplest animals, consist alone of this structure. Let us observe how nearly this vesicle agrees, both in its constitution and mode of action, with the vesicle of (80). Like that, it is not only an imperforate cell, but also consists of similar elements. The properties which the organized vesicle is supposed to enjoy are met with in the fullest extent in that which is not organized. Both have powers of endosmosis, and a species of assimilation of things exterior to their own substance. What property has the lowest order of animal and vegetable life which that bubble does not possess? A thing thus endowed with vitality may well excite our interest; it breathes, it is nourished, it exhales.

105. By referring the phenomenon of endosmosis to absorption, such as has been recognised by chemists, we advance one point in the simplification of our knowledge. It gives us also a better idea of the specific action of tissues, as depending on structural arrangement, and presents an intricate problem in its easiest form for solution; moreover, it is, as I know by experience, a safe guide in experimental research. We can hardly doubt that the forces bringing about the result indicated in (80) are the same as those which operate in Dr. Mitchell's experiment, where India-rubber is used as a barrier; and if that result receives so ample and so easy an explanation upon this doctrine, why should we hesitate to apply it to the other? But the composition, structure, and habitudes of a thin, watery film, are much better known than those of a lamina of India-rubber: we can reason with certainty respecting the one, and vary its composition to suit the purposes of experiment; the other affords no such advantage. If, however, it should eventually be found that the simple doctrine of absorption is not sufficient to explain all the phenomena of endosmosis that may hereafter be discovered, this paper will at least prove that the cause of those phenomena is not alone enjoyed by *organic* and *solid* tissues, but also by *liquids* and *substances without organization.*

CHAPTER V.

THE PHYSICAL THEORY OF CAPILLARY ATTRACTION.

(*From the American Journal of the Medical Sciences for February,* 1838.)

CONTENTS: *Importance of Capillary Attraction in Physiology.—Capillary Attraction is an Electrical Phenomenon.—Its Physical Theory.—The Effect varies with Variations of Electric Disturbance.—Takes place between Bodies of different Forms.—Physiological Illustrations.*

106. IT has been alleged, as a bar to all physiological investigation, that the phenomena of life are of so peculiar a nature, that we must necessarily forever remain ignorant of their causes; that, unlike physical phenomena, which are of a simpler kind, and more within the reach of human understanding, there is something in these inherently myste-

rious and incomprehensible. This unphilosophical impression exists not only in the minds of the vulgar, but has extended itself to men well trained to scientific research; it is to be found in the writings of the most eminent physicians, and often affords a plausible screen for professional ignorance. Of all the sciences, medicine is the last to profit by the analytic method—a method which has raised other departments of knowledge to their present rank. Its cultivators pursue the same course of synthesis which was pursued in the days of the Greeks—they reason from hypothesis to fact, instead of from fact to hypothesis.

107. It may, however, be boldly declared, that the science of life is not more occult than any other of the sciences. We may, by proper investigation, carry it as far; and we shall only stop short at the very same point which has proved impassable in them. Of final causes we know nothing; the immediate agent of life is not more obscure than any of the remote physical agents. If we cannot assign any reason why a seed germinates, can we tell why a stone falls to the earth? is the one phenomenon any more comprehensible than the other? If we cannot tell how it is that one parent should produce a countless offspring, each of which has the power of reproducing beings like itself, neither can we tell how a spark produces an extensive conflagration. It avails us little to say that the principle of life, like the principle of heat, possesses a radiant character, or has a power of self-production. We are equally ignorant how the wide-spreading flame results from a spark, and how countless myriads of seeds have originated from one primordial germ.

108. Some parts of the science of physiology are doubtless within the reach of scientific investigation. Most of the functions of organic life are of this character. Absorption, secretion, circulation, and respiration are carried on through the medium of tubular arrangements of different kinds, endued with specific powers. We are not well informed of the nature of these actions, nor of the force giving rise to them. The changes taking place in organic structures partake partly of a mechanical, partly of a chemical aspect, bearing some similarity to other physical changes effected by known agents, yet not identical with them. Some have supposed that the attraction of affinity, or the force of capillarity, was the power in question, operating in an unusual manner, under unusual circumstances; but the majority of medical writers have cut the knot, instead of untying it, and assert that it is a peculiar force, recognised under the title of vital force, life, or nature.

109. It is, however, most unphilosophical to resort to these vain explanations, which, after all, afford us no information, substituting only obscure terms as the causes of events not more obscure. Had we approached the problem of pore-action in the same spirit that has led to the development of the causes of magnetic action, a similar and equally striking advance would have been made.

110. Capillary attraction, considered simply as a mechanical force, is not competent to produce those changes which the pores and narrow cylinders of organic structures give rise to. The products of glandular action are chiefly compounds of a definite number of equivalents, bearing a strong resemblance to the products of ordinary chemical action; but still the operation of capillarity as a force producing motion is undeni-

able. Can it also produce chemical changes? Is it simply a manifestation of the electro-chemical relations of matter?

111. Previous to entering at large into an examination of the laws of pore-action, this query will demand an answer. We shall find, from what follows, that capillary attraction is a force nearly allied to, if not identical with, chemical affinity. Now the investigation of the problem of pore-action naturally divides itself into two parts. 1st. The mechanical conditions of equilibrium and movement of fluids residing in tubes of narrow diameter, but of any length. 2d. The chemical changes which fluids so situated undergo.

112. The identification, therefore, of the force producing the mechanical effect, with that producing the chemical changes, is a most important point, and to this I shall direct my attention.

113. There are two phenomena of capillary attraction, the conditions and circumstances of which are well known: the rise and depression of fluids in tubes of a certain diameter, and the adhesion of flat solid plates to the surface of fluids. From the former of these this kind of attraction has derived its name; the latter furnishes us with the means of making researches, devoid of ambiguity, in reference to the physical cause of capillarity.

114. If a circular disk of glass, or any other solid substance (*fig.* 15, *pl.* 1), *a b*, be placed on the surface of any fluid, *e f*, by means of a handle *c*, it will adhere thereto with a certain force, which may be measured by means of a balance, but which is sufficiently evident when attempts are made to lift the disk with the hand. This force is known under the name of capillary attraction. An investigation of its physical cause, and the laws respecting it, involves the fundamental propositions of pore-action and passage through tissues.

115. The phenomena of capillarity are brought about by electricity operating under peculiar circumstances. They are due to a disturbance of the electric equilibrium, and hence are intimately allied to all kinds of chemical and vital changes.

116. Let *a b* (*fig.* 16) be a glass plane reposing on the surface of mercury, *c d*, contained in an insulated vessel, and capable of being elevated by an insulating handle, *e*; let the mercury be connected with an electrometer, *f*, by means of a wire. Now, so long as the glass plane and the mercury are in contact, the electrometer evinces no disturbance; but as soon as the plane *a b* is raised by its insulating handle, electricity is instantly developed, and the gold leaves diverge. As there was no electrical excitement while the plane and the metal were in contact, it is a legitimate inference that the electricity now developed was the cause of their strong attraction or adhesion; and this is corroborated on taking the glass plane to another electroscope, when it will be found that it is electrified positively and the mercury negatively; and that, consequently, when they are brought into the vicinity of each other, a powerful attraction *must* result.

117. A cause of attraction being thus developed, it would be very unphilosophical to seek for other agencies where one so competent to produce all the effects is observed to exist. For in every case where a solid plane reposes on the surface of a fluid not wetting it, a large amount of electricity of very high tension is produced, the electricity

of the surface of the plane being always opposite to that of the liquid. They *must, therefore, attract each other.* I express here only a fact, not involving any disputed hypothesis whatever, as to whether that development of electricity originates in the mere contact of the bodies, their chemical action, or any other cause; but it is a fact, that when any solid reposes upon any fluid, provided its surface does not become wetted, a development of electricity uniformly takes place, and a powerful degree of attraction must necessarily ensue.

118. The postulate here introduced requires explanation, for electric excitement is not observed if the solid surface is wetted. Solids bear a peculiar relation to liquids, being wetted or not wetted by them. Most solids, for instance, are wetted by water, and but few by mercury; the surface of the glass is readily moistened by alcohol or oil, but not by melted sulphur or mercury; hence the latter, from its not adhering to the skin, was called by the older chemists *aqua non madefaciens manus.* The circumstance that no electrical excitement is observed when a solid surface is wet, might appear, at first sight, contradictory to the hypothesis here assumed. A more accurate examination, however, places it in a very different light, and shows that the phenomena observed are exactly such as they ought to be hypothetically. If a disk of glass is placed on the surface of the water and then removed, the gold leaves of the annexed electroscope are not affected, for, strictly speaking, no rupture has taken place between the solid and the fluid; the thin film of the latter in contact with the former still remains so: it is only the cohesion of the watery particles that is overcome, not the adhesion of the solid to the fluid, and hence no electrical development appears.

119. Geometers have shown the exact relation a solid must bear to a fluid to be wetted by it. It results from the mathematical investigations of Clairaut, that if the attraction of the particles of the solid for those of the fluid is more than half the attraction of these last for each other, the solid will be wetted; but if it be less than half, the solid will not be wetted. An experimental proof of this may be obtained by counterpoising a disk of glass, at the end of one of the arms of a balance, by weights in the scale, and then lowering it on the surface of some mercury in a cup; it will be found that a certain weight must be added in the scale to detach it. Next, in place of the disk of glass, substitute a plate of *amalgamated* copper, of the same size and weight, and ascertain the force required to detach it; this will uniformly be found more than double the former weight. The first weight expressed the attractive force existing between a surface of glass and mercury; the second, the cohesion of a cylinder of mercury of the same diameter, and the numbers obtained experimentally corroborate the investigations of Clairaut.

120. I dwell on this part of the phenomenon because it is of no small importance; the same conditions that determine whether or not the surface of a solid is to be wetted, determine also whether a liquid shall pass through a pore, and move forward in a capillary vessel.

121. The difficulty arising from the non-development of electricity, where the solid surface is wetted, being thus dismissed, we next inquire whether the hypothesis here assumed will give numerical results analogous to those procured by experiment; in

other words, if two solids which adhere to a certain fluid, with forces differing in amount, develop upon rupture quantities of electricity in the same ratio. As a general result, the balance and electrometer prove that this is the case. Bees' wax, which adheres to mercury with much less force than gum lac, develops likewise much less electricity. Gum lac, which adheres less strongly than glass, likewise develops much less electricity; but when we attempt to run a comparison in this manner among a series of substances, we find there are many disturbing causes which, in most cases, incapacitate us entirely from making comparable results. Much depends on the relative conducting power of the surface employed. A plate of iron may be separated from a surface of mercury, which does not wet it, with a very small disturbance of electric equilibrium, arising from the high conducting power of the metallic plate, which enables a transfer of any free electricity to take place if the plate should tilt on one side, or anything affect its horizontality during the act of separation. In proportion as the conducting power increases, although the force of adhesion may remain the same, the total effect on the electrometer should diminish; and this is agreeable to experience. Again, the presence of moisture on any part of the touching surfaces will vitiate the results; partly owing to its high conducting power, but chiefly to the circumstance that it hinders the surfaces under trial from ever coming into contact.

122. The circumstance of this great variability in the amount of developed electricity is in itself strong evidence of relationship between the supposed cause and the effect. Gay-Lussac found that it required a weight, sometimes of 158, and sometimes of 296 grammes, to detach a certain disk of glass from mercury, depending on causes which were not very apparent. An effect thus differing in amount indicates a cause of like variability, or subject to many disturbances.

123. I regard, therefore, the agent bringing about capillary phenomena as identical with that producing chemical action, and both as being due to electricity. The force of cohesion bears the same relation to both, acting on both as a disturbing power. Nay, we may even take a much more extensive view of the matter, and from the ratio these forces bear to each other predicate the effect of their combined action, which may be classed under three distinct heads.

1st. If the force of attraction of the particles of a solid for the particles of a fluid be not equal to half the cohesive force of the latter for each other, the fluid will refuse to pass through a pore of that solid substance, and in capillary vessels consisting of it will be depressed below its hydrostatic level.

2d. If the force of electric attraction of the particles of a solid for the particles of a fluid exceeds half the cohesive force of the latter for each other, but is not equal to the whole force, the fluid will pass through a pore formed of that solid substance, and in a capillary tube of it, will rise above its hydrostatic level.

3d. If the force of electric attraction of the particles of a solid for the particles of a fluid exceed the whole cohesion of the latter, *chemical union* ensues.

124. In thus assimilating the force producing pressure on planes and motion in narrow pipes, with the force producing chemical changes in the constitution of bodies, a great advantage is gained in simplifying physiological investigations in respect of the

action of capillary systems. It is an electrical force that determines all kinds of constitutional changes developed in bodies by the chemistry of organic life, and it is a manifestation of the very same force that carries some fluids along the almost invisible vessels of living structures, and denies to others a passage. All the phenomena of inorganic chemistry are the result of the balancings of the force of cohesion on the one hand, and electrical attraction on the other. If Berthollet was wrong in supposing that chemical affinity, as an acting force, had no existence, other chemists have equally erred in supposing that all kinds of changes, without any limitation, were due to it. Whether we investigate the phenomena of chemistry or of capillarity, we have the same forces to deal with, acting as antagonists to each other; and hence the whole effects imputed to capillary attraction may be regarded as belonging to that extensive class which the science of chemistry considers.

125. There is a variety of facts recorded by writers on capillary attraction, which an application of these principles readily explains, though hitherto they have been regarded by philosophers as remarkable anomalies. Such is the observation of HUYGENS, that it was possible to cause mercury to stand in a barometer seventy inches high; or that of P. Abat, of a singular deviation in the hydrostatic level of the same fluid in different branches of a siphon.

126. Capillary attraction does not take place only between solids and fluids; it is exhibited when solids alone are made use of. In virtue of this power, two pieces of lead cohere with great energy to each other, as also is the case with two planes of polished stone or plates of glass. When glass is used, electricity of very high tension is readily detected, one of the pieces being positive and the other negative; it would, I suppose, hardly be denied that the force operating in the case of glass is also the force that operates in the case of stones. Is it not, then, a legitimate supposition, that the adhesion of two pieces of lead is brought about by the same agent, the presence of which is masked by the high conducting power of the metal?

127. Between solids and gases capillary action likewise takes place. On the surface of all kinds of solids atmospheric air remains in a state of condensation, as is made evident when such bodies are placed beneath water under an exhausted receiver; the air appearing in copious bubbles, studding the surface of the metal.

128. Now having a power the operation of which over inorganic masses is so extensive, it is for us to inquire how far the phenomena of organic systems depend upon its working. Those numerous pores, and pipes, and capillary vessels which abound in all kinds of living structures, but of the action of which we are so ignorant, point out to us capillary attraction as one of the great forces in play, determining all kinds of motions and physical changes. To identify the force producing motion of a mechanical character with that effecting physical change, gives a unity to the action of powers which have hitherto been multiplied without avail, and stamps simplicity and symmetry on actions that are very diverse.

129. It is not alone in the vital functions that we meet with applications of the principles of capillary action; the mechanical functions furnish numerous instances. The organs of progression of some animals which delight to walk upon water are provided

with an apparatus of hair, calculated to repel that fluid; hence gnats and certain other insects have no difficulty in passing over the surface of water. By the same means the hydra suspends itself, without effort, in that element; for having exposed for a time the extremity of its foot to the air, so that it may become dry, it, by repulsion, forms a cup-shaped hollow around it, the head of the insect hanging down in the water beneath.

130. Organs of exhalation and absorption are unquestionably capillary systems. The stomata of plants, which botanists suppose to discharge these functions, are of this character; they furnish a well-marked instance of the accommodation of apparatus to suit physical conditions. Plants growing beneath the surface of water have no stomata; but if, by any means, they reach the atmosphere and vegetate in it, these organs are produced for the purpose of discharging, under the new order of things, offices which were accomplished by other means. The spongioles of roots, acting as capillary systems, drive the fluids they absorb from the earth, through the tubular vessels of trees, with a force of several atmospheres, extending themselves at a due distance from the trunk, where they may meet with the water that falls from the leaves. In some orders of living things, which are not accommodated with distinct orifices for the reception of food, nutrition is accomplished by capillary systems. In this manner the *porifera* expose a wide surface to the seas, and draw in nutrient matter through their microscopic pores, discharging the surplus, as excrementitious matter, through their papillary orifices.

131. Like the lungs of the mammalia, the leaves of trees are respiratory organs, composed of capillary systems; their mechanical functions are not so complete, though their chemical functions may be identical. They demand no nervous cords to be spread upon them to give them motion and keep up their play; the breezes in which they tremble perform the office of carrying off the exhaled impurity; and the rays of the sun furnish them with their vital force, enabling them to effect the decomposition of carbonic acid, and provide a store of carbon for the purposes of the economy.

132. In identifying the mechanical with the chemical force of organic structures, we see another proof of that unity of design existing through the entire range of living things. Functions of all kinds are accomplished by arrangements of every sort in different classes, yet no one will deny that they all follow one original type. Digestion, as it takes place in the stomach of man, appears a highly complex phenomenon, depending, as some say, partly on the tissue action, partly on nervous, and partly on other powers. But are not analogous changes wrought without all this complexity of apparatus in the hydatid, which may be taken as the elementary type of the stomach; or in the tænia, which is a colony of stomachs? The polygastric infusorials, some of which have hundreds of these organs, and even the mammalia, do not digest more perfectly than the hydra, a carnivorous polypus, which may be turned inside out without detriment. The laws of digestion followed by the one, are followed, too, by the other. If the organ of the one respects the presence of living matter, and refuses to act on it, so does that of the other; yet the one is furnished with a highly complicated assemblage of muscular bands, of glandular apparatus, of bloodvessels, of nerves, and the other is not.

133. In the higher orders of life, processes are carried on by multiplied apparatus, without, however, deviating from the principle of the original simple type. The gift of

a new faculty, or the addition of a new organ, brings with it a corresponding change in the arrangement of the whole plan. An engineer, who wishes to adapt a machine to the execution of some new task, alters every part, no matter how remote it may be from the acting point, until every wheel and lever executes its work co-ordinately with all the others; the prime mover remains unchanged, though the general character of the machine may have undergone a renovation; and as all machines, no matter of how many parts they are composed, nor of how many wheels they consist, nor how intricate soever may be their resulting motions, may have their power reduced to and represented by a simple lever, so also organic functions, though often brought about by highly complex arrangements, find simple representatives in the lower orders of life. A concentration, or a development of any organ, is often demanded by change in a remote part of the fabric, when even the connexion may not be very evident. Animals, consisting simply of digesting cavities, require no vascular system for propelling or containing a nutritious fluid; they are not in need of separate tissues devoted to its oxygenation, nor of an insulated respiration, nor do they demand distinct biliary organs; when the nutritious chyle is produced in the stomach of zoophytes, it finds its way into the inter-cellular spaces, and there circulates without vessels, undergoing through the external tegument the chemical changes. In many insect tribes, the bronchial tubes are spent upon the peritoneum, and respiration takes place directly upon the alimentary canal. With modification of functions, change of external figure is always involved; and as these progress together, systems of living things are constructed, referrible to one common original type. It is thus, in the echinodermata, we trace up successive steps, from the sea urchin to the asterias, and from that to the pentacrinite; a development of the same parts of the structure continually taking effect, until the extremes bear no sort of resemblance to each other.

134. Had the production of living things been effected by the operation of second causes, we might look, with Lamarck, for some law of successive development which should contain the origin of each order and species. We might regard the rudimentary teeth of whales, or the sub-cutaneous feet of the ophidia, as abortive results of such a law. Considering the brain as a development of the spinal axis, we might trace in the form of the cranial bones a development of a system of vertebræ, brought about as a consequence of the very same laws. We might run a parallel of analogies between the crustaceous and vertebrated animals, and exogenous and endogenous plants; we might take the cephalopodous mollusks as furnishing the first rudiments of an internal skeleton, and trace its increasing complexity to meet certain ends until its perfect development in the mammalia. In this latter class, we might dwell upon the uniform existence of seven cervical vertebræ, as giving evidence of a persistence in the plan of structure in species so remote from each other as the camelopard, the whale, and the mole. Parting from the dorsal vessel of insects, the first rudiments of an aorta, we might follow out the complications of the higher arterial systems. In all the varieties of respiration, whether aquatic, aerial, or mixed, we might see the reproduction of one original chemical design, and in every instance of a concentration of machinery or functions, we might find an impress of the action of external formative agents.

CHAPTER VI.

ON THE GREAT MECHANICAL FORCES GENERATED BY THE CAPILLARY ATTRACTION OF CELLULAR TISSUE.

(*From the American Journal of the Medical Sciences for May*, 1838.)

CONTENTS: *Physiological Relations of Cellular Tissue.—Force with which Gases and Liquids pass through Cellular Tissue.—Disturbing Action of Leakage.—The Capillary Force overcomes powerful Mercurial Pressure.—Dalton's Hypothesis.—The Tissue is the Origin of the Force.—Its Absorbent and Condensing Action.—Voltaic Batteries may be used for producing great Pressures.—Gases pass when resisted by the Force of many Atmospheres.—The condensed Gas acts as a Vacuum.—Co-ordination of the Results of Dalton, Graham, and Mitchell.—Disturbing Agencies.—Disturbance by Variation of Temperature.—Physiological Experiments and Remarks.*

135. OF all bodies, those alone are capable of exhibiting the phenomena of life which consist of a cellular structure. Identity of chemical constitution does not appear to be essential, yet it is only a limited number out of the long list of chemical elements that are capable of organization; these, if left alone to satisfy the conditions of their affinities undisturbed, would most commonly give rise to the production of water, ammonia, and carbonic acid. Life, therefore, in this point of view, has no other action than to disturb the play of these affinities, and force the elementary atoms into other forms of combination; it depends upon the success of this action whether a living or inorganic mass shall result. A living body is endued with a peculiarity of form, and does not require an identity of composition; an inorganic body depends for its nature on certain and definite composition, without any relation to structure. It is true that most bodies, whether elementary or composite, exhibit a marked tendency to geometrical arrangements, and all crystallizations are brought about by the operation of polar forces, but an inorganic compound body does not of necessity require any peculiar crystalline shape, or other form, for existence.

136. Life, then, is a state of force; the system of nature presents us with but four of the chemical elements subject to it, for we are taught to make a distinction between crystalline arrangement and living structure. We have not any direct evidence to show that all simple substances are in any wise obedient to the laws of vitality, or that, when they assume symmetrical arrangements round an axis, that it is an approach to organization, an imperfect organization depending on the sluggishness of their character or the incompetency of the vital forces to control the range of their affinities; nor is there any proof that the laws directing the atomic arrangement of macled crystals bear any sort of analogy with those that direct the structural deposite of the radiated class of animals. It is true that the passage of a polarized ray of light through transparent crystals has disclosed to us the fact that their atomic constituents are held together in a state of

force, and we judge from the phenomena of their nodal lines, when they are thrown into vibration, that their elasticity varies in different parts; yet the mere fact of their permanence assures us that they are in a state of stable equilibrium. On the other hand, organized structures are in a condition of instable equilibrium, and require a continued series of adjustments for the perpetuation of their existence. In the crystal, the electrical or polar forces have compensated one another, and its particles being brought into a state of rest, continue so without change; while in the living being their situation is only momentary; they are subject to incessant vicissitude and change; their place has to be supplied by new material; and to accomplish this end, electrical currents traverse the body in all directions, and machinery more or less complex is employed to bring new matter and carry the effete away.

137. Does this cellular or areolar structure, which appears to be the essential habitat of vitality, owe its properties to the residence of a peculiar force, or are they derived from its organization? If the latter, we ought to find it possessed of remarkable characteristics, of forces arising from the aggregation of particles.

138. It has been known for some years that gases and liquids pass through porous structures with a considerable force. If over the mouth of a cylindrical jar a thin sheet of India-rubber is tied, and the jar exposed to an atmosphere of ammonia or protoxide of nitrogen, in the course of a short time, by the ingress of a portion of the external atmosphere, a pressure is created tending to rupture the membrane outward.

139. That the force exerted in this case is very great, appears from the following experiment: In a cylindrical jar, *a b c d* (*fig.* 17), four inches long, and one and a quarter in diameter, a siphon gauge, *e*, was placed, and over the mouth of the jar a piece of India-rubber fortified by a layer of stout cloth was tied. Two pieces of tape, crossing each other at the top and passing down the sides of the jar, were knotted as tightly as possible at its bottom, and the arrangement was then exposed to an atmosphere of ammonia. In the course of six hours, the India-rubber, notwithstanding it was forcibly held down by the cloth and tapes, began to stretch upward, and the gauge had risen thirteen divisions of an arbitrary scale attached to it. In twenty-four hours it had risen to nineteen and a half, and, finally, to twenty, after which it remained stationary. On estimating the divisions of the scale, after the experiment was over, it was found that the maximum pressure in this case was about two thirds of an atmosphere, or ten pounds on the square inch.

140. This effect is not confined to gases, but takes place with equal energy when liquids only are used. In a jar containing alcohol, a gauge was placed, and a piece of human peritoneum was stretched over the mouth, fortified by silk. The whole was then sunk into a vessel of water. In twelve hours it was found that the level of the fluid in the gauge had risen the whole length of the scale, and that when the maximum pressure took effect, the gauge was exhibiting a condensation of one atmosphere exactly.

141. Here, then, we have proof that the passage through tissues is accomplished with a degree of energy indicating that the forces which produce it are of a very high intensity. To measure these forces, or to obtain some approximation of their value, the following researches were made.

142. Before, however, proceeding to the detail of these experiments, it is necessary to allude to certain disturbing circumstances which take place, arising from extraneous mechanical action, and vitiating the result. One of the most prominent of these is due to the general leakage which happens through the open pores of all tissues; a leakage which is to be distinguished from the proper capillary transit. If, for example, a barrier of peritoneum be placed over the mouth of a vessel of water, under ordinary circumstances the escape of the water will be prevented; but if a pressure gradually increasing be exerted on the water, it will rapidly ooze through every pore, and finally, if the membrane stand the strain without rupture, will spirt through those of a large diameter. This effect, to a greater or lesser extent, takes place wherever tissues have to resist mechanical pressure; the amount of disturbance arising from it depends mainly on the diameter of the pores of the structure.

143. In the experiment related in section 139, we have a well-marked instance of this disturbance. It might be inferred from that experiment, that the force with which water passed through a piece of peritoneum into alcohol was not greater than one atmosphere; whereas, in truth, it was much more; but, as soon as the pressure within the vessel by the infiltration of water had amounted to about one atmosphere, the alcohol escaped from the vessel as rapidly as the water entered, by general leakage from the whole surface of the membrane, and the gauge, therefore, gave no evidence of the passage of the liquid. Nor are very porous structures alone liable to this accident; the experiment of the old Florentine academicians shows, that even through the pores of gold, one of the densest of the metals, fluids under a severe pressure will find their way, as appeared when they attempted to compress water in a globe of that metal.

144. This accidental passage through pores may be made visible to the eye by condensing about one atmosphere of air into a vessel whose mouth is closed by a sheet of India-rubber, and then placing it in a jar filled with water; small bubbles of air will be seen escaping from every part of the India-rubber, and passing in great numbers through the water.

145. It has just been stated, that the force with which water passes through a membrane into alcohol is much more than one atmosphere; this may be proved by making use of a barrier of a stouter fabric than the peritoneum here mentioned. A piece of bladder being used in lieu of it, the gauge indicated, when the pressure was a maximum, a force of 1·8 atmospheres; but even this cannot be taken as the true value of the force, for a certain period of time elapses, amounting, in this instance, to almost two days, before the gauge reaches its highest point; and when that is gained, the alcohol has become considerably diluted, and agreeably to a law which will hereafter be pointed out, the amount of force rapidly diminishes as this takes effect; for, as soon as the composition of the fluids on both sides of the bladder is the same, provided the temperature of both is alike, and no mechanical disturbance arises from unequal pressure, all motion either way ceases, and this may happen long before the column of fluid in the gauge has reached its highest point.

146. The air gauge, however, at the best, is a very imperfect indicator of the force with which gases or liquids mingle, for it will remain stationary, even when the passage

is taking place with very great force, provided the rate of the bodies on both sides of the barrier is the same. It gives erroneous results in all those cases where the mechanical leakage exceeds the true percolation, and hence has a very limited application in all these experiments. Other means are therefore required to test the passage of fluids, and for this purpose there is no arrangement more convenient than that represented in fig. 18. It consists of a tube, three eighths of an inch in diameter, and several feet long, bent at the point *b* upward, and expanded at *c c′* into a funnel termination. When the instrument is in action, the longer limb, *d b*, is filled to some determinate height with mercury, which also rises to a certain distance in the shorter leg, above this, and to the height *a a′* some fluid is placed acting as chemical test of the presence of the gas, intended to be passed through the barrier *c c′*, which is tied air tight over the funnel mouth. The following experiment will indicate its use: Having placed this siphon on the mercurial trough, a quantity of mercury was poured into it, sufficient to cut off communication between the two limbs; then in the shorter limb a column of litmus water reddened by muriatic acid, and occupying a depth of one eighth of an inch, was introduced; over the funnel mouth a thin lamina of India-rubber was tied, and upon that a piece of stout silk, for the purpose of strengthening the barrier. A column of mercury, forty-three inches in height, was next placed in the long limb, and a jar of ammoniacal gas over the short one. In the course of one minute, a cloud of dark-blue particles was seen descending through the litmus, and in six minutes it had become uniformly blue; thus proving the passage of ammonia through a tissue of India-rubber, against a pressure of almost one atmosphere and a half.

147. There were considerable difficulties encountered in the outset of these experiments in tying on the India-rubber barriers, so as to withstand the high pressures to which they were exposed without leakage; an insidious leakage which took place between the sides of the glass and that part of the India-rubber compressed by the string against it. This, however, was effectually prevented by setting fire to a piece of India-rubber, and daubing the semi-fluid material on that part of the glass around which the string was to pass; then, on tightly binding on the barrier, it came into perfect contact with the glass, and was retained there by the sticky material, no leakage whatever taking place, unless some part of the arrangement burst.

148. The experiment just related leads to some important conclusions; we see that the force of impulsion driving ammonia into atmospheric air exceeds a pressure equivalent to forty-three inches of mercury, the barometric pressure at the time being 29·73, that is to say, exceeding by very near half an atmosphere the force which theory would indicate. The hypothesis of Mr. Dalton, which seems to me to be fully confirmed by the observations of Mr. Thomson, founded on the experiments of Mr. Graham, assumes that gases act towards each other as vacua, or, in other words, the force impelling the particles of one gas into the interstices of another does not exceed the barometric pressure; but here we find that the result apparently leads to a very different conclusion. It was from an experiment of this kind that Dr. J. K. Mitchell was led to doubt the truth of Dalton's theory, inferring from his results that gases penetrated each other with much greater force. Such a conclusion, however, does not legitimately

follow, for it is highly probable that the nature of the barrier itself is very much concerned in the final action. A gas may penetrate into another with a force not greater than one atmosphere, and yet, because of the disturbing agency of the medium through which it must go, it may succeed in lifting a column of mercury equivalent to a pressure of many atmospheres.

149. The evidence proving that gases do not infiltrate each other with a pressure greater than one atmosphere is very cogent. Much of its weight is derived from the identity of the resulting volumes of commingled gases; but the most important fact relates to the passage of these substances into each other, when the barrier separating them is very porous and has no condensing action, as is the case with a stucco plug, which opposes simply a mechanical impediment to their motion, acting, as will be hereafter proved, merely as a temporary valve; a mode of action totally different to that of closer textures. The final volumes exchanged being inversely proportional to the square root of the densities, and these final volumes representing the true initial velocities, we have a striking illustration of that law of gaseous mechanics, that the velocities of different gases, rushing into a vacuum, are inversely proportional to the square root of their densities. Consequently, we are constrained to infer that one gas acts towards another in the same manner as if it were a vacuum; and, therefore, that the force impelling the particles of one gas into the interstices of another never exceeds the pressure of one atmosphere.

150. In an experiment made on the passage of ammonia into atmospheric air, it was found, that though the passage of the gas was resisted by a pressure of seventy-five inches of mercury, or upward of two atmospheres and a half, it took place, apparently, as readily as if no such resistance had been opposed to it. The question at once arises, Whence is this powerful impulsive force derived? clearly not from the action of one gas upon the other, for there is great probability, as we have already seen, that that force would not be able to lift more than thirty inches of mercury. The porous tissue or barrier *alone* can be regarded as the seat of this power. This fact, that systems of capillary tubes, or thin tissues, have in themselves certain powers, capable of producing high mechanical action, and operating successfully against the severest pressures that can be brought to bear against them, is worthy of the serious contemplation of physiologists; it is a great error to impute the forces producing these phenomena to the gaseous media. In the tissue itself we must admit a source of power, a source far transcending that which solicits the gases to penetrate each other. Let us next inquire into the nature of this power.

151. It is well known that porous substances of all kinds and fluids absorb gaseous matter very readily, in volumes varying according to circumstances. Water, for example, absorbs its own volume of carbonic acid, and 480 times its volume of hydrochloric acid gas. In the latter case, therefore, an extremely great condensation takes place. So, too, a fragment of porous charcoal absorbs nearly ten times its volume of oxygen, and ninety times its volume of ammonia; these gases, therefore, exist on the surface of the particles of the absorbing medium, in a state of very high compression. And the reasoning which here applies, applies also in the case where the two gases are separated by

a tissue. If, for example, we separated, by a medium of this kind, a certain volume of ammonia from a like volume of nitrogen gas, though at the outset of the experiment both the gases might be existing under the same pressure, yet this equality would very rapidly be lost. The absorption of the ammonia taking place with much more rapidity than the nitrogen, it would be presented to this latter gas, not under an equivalent pressure, but in a state of great condensation. Under such circumstances, the transit of a gas is not, as will be shortly shown, analogous to the case where it flows under common pressure into a vacuum, or into another gas, but the tissue, continually acting as a perpetual condensing engine, brings the two media in contact with each other under extremely different conditions; the one in a compressed state, but ready to exert the whole of its elastic force, the other in a state perhaps little varying from its normal condition.

152. If tissues really exert a power of this kind, some might inquire how it is that, when a tube closed at one end with such a structure, and filled with mercury, is sunk in the trough to its hydrostatic level, atmospheric air, or any gas to which it is exposed, does not pass through and expel the mercury from the tube. If, it might be said, the gas is existing in such a condensed state in the tissue, what is the reason it does not expand, and drive the mercury down? Experiment proves that this is not the case, but no argument can be drawn from it at all affecting the position here taken; for, as soon as the gas has gained the under side of the tissue, there is no cause soliciting it to escape any more this way than backward into its own atmosphere; the pressures each way are equal, and, therefore, counteract each other's effects; or, rather, the pressures are unequal, for that tending to expel the mercury is resisted by the hydrostatic action of that fluid, and hence no gas can pass into the tube.

153. We can now understand the rationale of action in Mr. Graham's experiment with plugs of stucco. He found that this material exerted a very slight absorbent power over the gases; oxygen, hydrogen, nitrogen, &c., not being absorbed in any sensible quantity. When, therefore, he diffused hydrogen into atmospheric air, the stucco not acting mechanically on either of those substances, they were presented to each other under equal and ordinary pressures, and they therefore began to flow into each other just in the same way that they would have flowed into a vacuum; but very different is the result when we make use of sheets of India-rubber or moistened animal membranes. The stucco plug serves only to make the experiment manageable by opposing a slight resistance to the escape of the gases, and acting, as I have said before as a temporary valve; so that, if a diffusion tube be fitted up in Mr. Graham's manner, at the end of the arm of a balance, the gas does not escape so rapidly but that there is time for a very accurate self-adjustment of the apparatus, and the volume of re-entered air can be measured with precision.

154. It might, perhaps, be objected to the view here taken, that the condensation which some gases experience is more than sufficient to liquefy them; and that, therefore, they do not act simply as gaseous bodies would do towards each other. This condition, however, when it does take place, appears not to change the resulting phenomena, as the following experiment shows. The thermometer being at 38° F., and

the barometer at 29·88, atmospheric air, under a pressure of two atmospheres and a half, was exposed, under a sheet of India-rubber, to sulphurous acid gas, care being taken that the temperature of the mercurial trough, and all parts of the arrangement, should be as above. The passage of the gas took place with great promptness, the litmus water, used to detect its presence, reddening rapidly. Now sulphurous acid, according to the experiments of Dr. Faraday, condenses into a liquid at 45° F., under a pressure of thirty inches of mercury; we know, therefore, that in this trial the gas must have existed in a liquid condition in the barrier, and yet it passed through into atmospheric air, under a resistance almost two and a quarter times sufficient to condense it, and at a temperature eight degrees lower.

155. Having progressed thus far in this part of the inquiry on the action of tissues, it became important to find if any pressure which could conveniently be brought into action would restrain the passage of gaseous matter. Resort was first had to the usual mechanical condensing apparatus; but they were found to be ill adapted to the purpose in hand. The necessary motions were always productive of inconvenience, and it was not found possible to carry the condensation to the degree required, or to avoid leakage from some of the numerous joints. After some trouble, the following contrivance was fallen upon, which answers the end perfectly, is not open to the serious objections of the former, and, requiring no cock or valve, can be readily made without leakage. A tube of glass about one third of an inch in bore, of stout substance, and about ten inches long, is bent into a kind of siphon, so that one leg shall be about six, and the other two inches long. The extremity, *a a*, *fig.* 19, has a lip or rim turned round it at the lamp; while in the longer leg, a thin glass tube, *c c*, about one eighth of an inch in bore, and closed at one end, is included to serve, as will be hereafter shown, as a gauge. Next, the extremity, *b*, of the siphon is closed, there being inserted through it two platinum wires, *d d*, *e e*, parallel to each other, but not touching. The arrangement is then ready for use. Suppose, for example, it was required to pass through India-rubber sulphurous acid gas into atmospheric air, condensed by a pressure of five or six atmospheres; the long leg of the siphon is to be filled with water, which is excluded from the gauge-tube *c c*, owing to the narrowness of its bore; next, a strong decoction of litmus is to be poured into the short leg until it is about half filled. The rim round the extremity, *a a*, is then daubed with a piece of burning caoutchouc, and upon it is tied a thin piece of that substance, with a fine but strong waxed thread. Over this is tied a piece of stout silk or cotton cloth, for the purpose of fortifying the barrier; the wires, *d d*, *e e*, are then made to communicate with the poles of an active voltaic battery, and the condensation commences; for the gas which is evolved from these electrodes rising to the top of the tube, accumulates there, causing the column of water in the short leg to rise and condense the atmospheric air above it. The membrane, though fortified, gives way to a certain extent, becoming convex outward; and as the accumulation of gas in the long leg continues, the condensation of that in the short leg increases, as is indicated by the gauge *c c*. A very thin India-rubber, of the diameter here used, will stand a pressure of from six to twenty atmospheres without rupture, if its silk support is good; and I have found that anointing the edges of the rim with the burned substance enables

the operator to tie it on so that no leakage shall occur between the India-rubber and the glass, even under the severest pressures. When the gauge shows that the required degree of condensation is arrived at, the connexion with the battery is broken, and the condensation, of course, stops; the siphon being carried to the mercurial trough, taking care to keep its position erect, its short limb is depressed under the mercury, and carried into a jar containing the sulphurous acid. If, under these pressures, any of the acid gas finds its way into the condensed air, its presence is detected by the reddening of the blue litmus water. It is necessary here to observe, that the indications of the air gauge do not give a correct estimate of the amount of condensation, but always represent them higher than they are according to Marriotte's law: it has long been known that the volume of gas dissolved by water depends, in a great measure, on the pressure exerted on it: now it will be found, when the operation is conducted in an instrument arranged as this, that a very large proportion of the air in the gauge disappears in this manner; its zero point is therefore altered, and the condensation appears higher than it really is. It may be remarked, in passing, that it is surprising to what an extent the absorption of oxygen and hydrogen is carried in the longer leg, owing to their making their appearance in a nascent form. To ascertain the true condensation, so soon as the passage of the sulphurous acid or other gas has taken place satisfactorily, the membrane is to be punctured with a pin, and when a pneumatic equilibrium is obtained, the height of the liquid in the gauge will mark the point where the zero of the scale should be placed.

156. Some might suppose that there is danger in making use of an apparatus like this, where a high pressure is produced, owing to the risk of an explosion of the compound gases in the long limb, since it is stated in most works on chemistry that a mixture of oxygen and hydrogen, when compressed, will explode. To ascertain if there was any danger arising from this, as also to know to what extent the condensation could be pushed by the aid of a voltaic battery, I took a tube, *a b* (*fig.* 20), and into the closed extremity having fused a pair of platinum wires, and drawn the other into a long capillary tube, bending it at the same time at right angles to the former, I filled it with water (boiled until all the air mechanically enclosed in it was expelled), except a portion of the narrow capillary part from *d* to *c*, which contained atmospheric air, to act as a gauge; the extremity, *c*, was closed. Next the platina wires were made to communicate with the poles of an active voltaic battery of 120 pairs, and gas slowly accumulated, the current of electricity steadily passing all the time, as was indicated by the deviations of a galvanometer, through which it was made to circulate. Observations were made every few minutes on the progress of the experiment, the last of which indicated a pressure of slightly upward of forty-three atmospheres, and shortly after it was taken the tube burst; not, however, on account of the explosion of the gaseous materials in it, but because it could not sustain so excessive a pressure tending to burst it, a pressure equivalent to that of a column of mercury nearly thirteen hundred inches high.

157. These results lead us to some remarkable conclusions in relation to the passage of voltaic currents. Dr. Faraday found that they cannot pass along such media as water without effecting its decomposition; in fact, that the transfer of elements seemed

to be absolutely essential to the transit of the electricity. Now it might be supposed that, if some powerful force were brought to bear against and antagonize this, as where, by a severe pressure, the oxygen and hydrogen are prevented from being evolved, one of four things must happen: 1st. That the water would become a non-conductor. 2d. That the vessel, no matter how strong it might be, would burst. 3d. That the current would pass without any decomposition happening; or, lastly, that the current would pass and gas be evolved, but as fast as evolved, it would be dissolved in the water. A quantity of boiled water was hermetically sealed up in a glass tube, which it filled entirely, except a small space occupied by a bubble of air, probably not more than one fiftieth part of an inch in diameter. A pair of platinum wires had been fixed into the tube so as to transmit the voltaic current. The current passing freely, as was indicated by a galvanometer, decomposition of the water ensued; extremely minute bubbles making their appearance, the water absorbing the greatest part of them, its temperature rising very much, so that the tube communicated a sensation of warmth when touched by the finger. When the pressure was estimated to have risen to about fifty atmospheres, the tube burst, and in an instant all the gas that had been imprisoned in the water made its escape, throwing it into a violent effervescence. Hence we find, that when water is enclosed hermetically in a vessel, and a galvanic current passes through it, decomposition ensues, a portion of the gases making their appearance in a gaseous form, replacing the small space occupied by the decomposed water, the whole of the remainder being absorbed by that fluid as fast as it is given off. When the pressure is high, it is probable that the dimensions of the vessel become greater, and hence the little bubble of air accumulated exceeds in bulk the volume of decomposed water. It is also found that any pressure up to forty or fifty atmospheres may be commanded in this way.

158. Being thus furnished with a very convenient and very portable method of condensation, I proceeded to examine the force of passage of gaseous matter into atmospheric air. Sulphurous acid passed instantaneously into atmospheric air, against a pressure equivalent to two hundred and twenty inches of mercury, or seven atmospheres and a third. Some experiments were made on the absorbing action of the sample of India-rubber here used, which had been softened in ether for the purpose of procuring it in thin sheets. Of the gas here spoken of, it was found to absorb sixteen times its own volume. It is to be expected that, even had a much more powerful pressure been applied, the gas would, nevertheless, have gone through.

159. The curved form of the instrument described in (155) was found to present certain inconveniences when pressures upward of six or seven atmospheres were made use of; the volume of air which, at the beginning of the experiment, occupied the greater part of the extent of the shorter limb, had now collapsed much in its dimensions, and owing to the unavoidable giving way of the India-rubber and its silk, had retreated out of sight beneath it. It was not found convenient to lengthen this limb, for that entailed a corresponding increase in the dimensions of the battery, in order to produce a given condensation in a given time; an objection also applying, in a measure, to the apparatus even at lower pressures. Though I had the command of batteries, consisting of six hundred pairs of four-inch plates, I preferred a modification in the instrument itself, than

a resort to such an energetic but unwieldy apparatus. A straight tube was, therefore, taken, about three sevenths of an inch in bore (*fig.* 21), and a rim turned on it at *a a;* at the closed extremity the platina wires, *b c*, entered, a gauge tube, *d d*, was dropped in between them, water was then poured in to the height, *e e*, and, lastly, a tube, *f*, containing the appropriate chemical test, was inserted, its bottom resting on the top of the gauge tube. Nothing then remained but to tie on the India-rubber, with its silken support, and by the voltaic battery to proceed to condense. In this instrument, the test fluid was never out of sight, nor did the volume of the gas suffer any inconvenient change; the gauge, too, was well located for observation, and a given condensation could be produced in less time, and by a less amount of electricity, than with the siphon tube; for the space contained between *a a* and *e e* was less in volume. As an auxiliary arrangement, a glass tube, *a a a a* (*fig.* 22), one inch in diameter and ten long, with a support, *b*, was taken, and its mouth ground true, so that a piece of plate glass, *e c*, would close it when placed over it; this tube served in many cases as a gas generator, and also as a receiver for the tube (*fig.* 22), which was dropped into it. It is to be observed, that in the arrangement here adopted, the gaseous matter evolved from water mingles with the atmospheric air in the upper part around the tube, *f*, and, therefore, the passage of the gases tried does not take place into atmospheric air, but into a mixture of oxygen, hydrogen, and nitrogen gases.

160. The tube *f* being filled with lime-water, and a pressure amounting to ten atmospheres being produced in the vessel, it was exposed to an atmosphere of carbonic acid, generated in the tube *a a a a*, *fig.* 22, procured by dropping a few pieces of marble into the tube, and pouring thereon dilute muriatic acid. When the vessel was full, the plate *e c* was laid upon it, and any surplus gas generated escaped by lifting it up. In the course of a few minutes, the upper part of the tube containing lime-water began to look milky, and in an hour, a cloud of particles of carbonate of lime had fallen to the bottom.

161. Again, having filled the tube *f* with a solution of acetate of lead, and produced a pressure amounting to twelve atmospheres, it was exposed to sulphuretted hydrogen, generated in the vessel, *fig.* 22, from protosulphuret of iron and dilute sulphuric acid. In a very short time, the black sulphuret of lead appeared, giving tokens of the rapid passage of this gas through the barrier. A comparative experiment was made, in order to discover whether the transmission took place more slowly than when it was not resisted by such a severe pressure. It appeared, however, so far as the experiment could be tried under similar circumstances, as regards the thickness of the barrier, &c., that sulphuretted hydrogen went through the barrier against a pressure of three hundred and sixty inches of mercury, as readily as if no such force were exerted against it.

162. As numerous experiments, which had been tried on various gases, had hitherto failed to indicate any obstacle to their passage, it became necessary to know whether, at the extremest pressures that could be commanded, they would pass through a barrier. To accomplish this, I took a strong and narrow tube, and, having turned a rim at one end and sealed fine platina wires in the other, I filled it with distilled water, and enclosed in it a narrow capillary tube, the gaseous contents of which were small. As

a test, in the upper part of the arrangement, and in lieu of the tube *f*, I placed a slip of paper, which had been alternately soaked in acetate of lead and carbonate of soda; the India-rubber was fortified by a piece of very strong silk, which was carefully tied on; there was not, therefore, any gaseous matter present except the small quantity of atmospheric air in the gauge-tube. The condensation, therefore, went on with great rapidity, a mixture of oxygen and hydrogen gradually accumulating in the top of the vessel, bulging out the India-rubber and silk barrier until it was almost hemispherical. It was my intention to try a pressure of twenty-five atmospheres; and when that was supposed to be reached, the instrument was placed in an atmosphere of sulphuretted hydrogen. Very soon the test paper became of a tawny appearance, and, finally, it was quite black. The pressure, when the experiment was over, was found to be twenty-four and a quarter atmospheres.

163. At a temperature of 48° F., and pressure of 29·74 B., sulphuretted hydrogen gas passes into a mixture of oxygen and hydrogen, though it may be resisted by a pressure of twenty-four and a quarter atmospheres, or nearly seven hundred and thirty inches of mercury. Like sulphurous acid, it penetrates through a barrier, and then diffuses into an atmosphere beyond it, at pressures greater than that which is necessary to condense it into a liquid.

164. If, as it thus appears, no pressure which we can command is sufficient to restrain one gas from passing into another, we next inquire what obstacle the condensed gas exhibits. There is abundant and conclusive evidence that, under ordinary circumstances of temperature and pressure, this medium bears the same relation to the percolating gas that a vacuum would do; inasmuch as the rate of discharge into it is identically the same as it is into a vacuum. For the purpose of illustration, we may, therefore, regard it to all intents as a vacuum, and reason accordingly. If the particles of heterogeneous gases possess no repulsive tendency as respects each other, but are perfectly quiescent and neutral; if the presence or absence of one makes no difference nor produces any retardation on the motions of the particles of the other, then it is apparent that it is immaterial how many of such particles are condensed together into a given space; owing to the want of repulsive action in those particles, that space will be as much a vacuum to any other gas as it ever was. Now it has been shown by the experiment above cited, that certain gases will diffuse into others, even though the latter may be condensed into a space twenty-four times less than that which they would ordinarily occupy. *The vacuum is not less a vacuum because it is contained under smaller dimensions, any more than a torricillian vacuum is less perfect when the mercury is made to rise nearly to the top of the barometric tube, than it was when there was a vacant space many inches in length.* Theory would therefore indicate that these diffusions will take place under all pressures, provided the gaseous condition subsists; and this conclusion is abundantly borne out by the experiments herein detailed.

165. Having thus shown how it is that, when gaseous matter is on one side of a barrier, the space so occupied may be regarded as a vacuum, even though the gas should be highly condensed, I come next to the consideration of a much more intricate part of the subject, the action of the barrier itself as an areolar tissue, which is the more imme-

diate object of this paper. I have already stated that the results of Dr. Mitchell and Professor Graham apparently exhibit a striking discordance; it will here be seen that the facts reported by those chemists can be readily co-ordinated.

166. Both of them appear to have made trials of the absorbent power of the barriers they respectively employed; Professor Graham having operated on a mass of stucco of certain dimensions, and found its absorbing power, in relation to most gases, very low; Dr. Mitchell on a thick cylinder of gum elastic; but neither of them appears to have clearly seen the importance of this element in the production of the final result. In the case of the action of stucco, this, indeed, is a remarkable circumstance, for in all those instances where the absorbing power of the stucco was great, the equivalent volumes of diffusion, as obtained, were, without exception, erroneous. Dr. Mitchell, on seeing certain gases pass into each other with a force that was greater than the pressure of sixty-three inches of mercury, and inferring that there was no *vis a tergo* in play, was obliged to impute his result to the inherent power of gaseous penetration; hence he came into direct collision with the Daltonian hypothesis. On the other hand, Professor Graham, supposing that, in all his erroneous cases, the deficit was to be imputed to the porous mass, which, in some manner, detained and absorbed the gases, found in every other instance a full confirmation of the doctrine of a vacuum.

167. The whole phenomenon depends, however, upon the action or inactivity of the cellular tissue itself; it will be convenient, for the better understanding of it, to consider it under two heads. First, where the tissue exerts no absorbent action on the media, or absorbs both to the same extent; and, secondly, where one is absorbed to a much greater extent than the other.

168. In the first case, the velocities with which any two gases pass into a vacuum are inversely proportional to the square roots of their densities respectively; moreover, the volumes that so pass vary directly as the velocities, and therefore may be taken as an index and measure of them; but, as the mass of each gas is expressed by the product of its density into its volume, it may be also represented by the velocity multiplied into the density; and, as the square of the velocity of the one, multiplied into its density, is equal to the square of the velocity of the other multiplied into its density, whatever may be the difference of the specific gravity of the two gases, their mechanical momentum will always be the same; the resistance they meet with in passing through the tissue is common to both, and equal in both cases; and hence the initial velocities of diffusion ought to be inversely proportional to the square roots of the densities; and as, during the progress of the experiment, the impelling force of the one gas is equal to the expelling force of the other, the resulting momenta of the two currents is still equal, and the final volumes are such as are found by direct experiment.

169. We now come to consider the second case, where the cellular tissue presents one of the gases in a condensed form to the other, or, in other words, absorbs it; and here we have to refer to a fundamental proposition of dynamics, that when the moving force and the matter to be moved vary in the same proportion, the resulting velocity will always be the same. An illustration will show the application of this principle to the case in hand. If a cylinder of air, fitted appropriately with a piston, communicates

with a vacuum by means of an aperture, it is immaterial whether the air be allowed to flow into the void without any pressure, or whether it be urged by a direct action on the piston, its velocity as it goes into the void will be in both cases the same; for, if it be compressed, the immediate action of the force exerted on the piston is to reduce the air in the cylinder to such a density that its elasticity shall be equal to the compressing force, and because the elasticity varies directly as the density, the density of the air increases with the impelling force. The matter to be moved is increased, therefore, in the same proportion with the pressure, and therefore the final velocity is the same. Now what is here said of a cylinder of compressed air, applies evidently to the action of a cellular tissue, which is nothing more than a perpetual and equable condensing engine. If it increases the elastic force of one of the gases by compressing it, at the same time it increases its density; and, therefore, its velocity of transit is the same as though it had not suffered any action of compression.

170. Such is the case while the gases are engaged with each other in the tissue; but as soon as they are passed from it, and are beyond the reach of its attractive force, a new condition of things takes place: the condensed gas being no longer under restraint, expands freely into a void, and when there measured, gives a resulting volume totally different to what it would have been had not the tissue compressed it. Suppose, for example, we placed on one side of a tissue carbonic acid, of which it would condense its own volume, and on the other atmospheric air, on which it exerted no action. While the two gases were engaged together in the tissue, one would be presented to the other under an elasticity double of that which it would have had had no absorption gone on; but since its density is directly proportional to its elastic force, the continual velocity with which it would rush into the other gas is the same as though no compression had occurred; the rate of exchange in the cellular tissue is the same as under normal circumstances; that is to say, every volume of air replaces 0·8091 of compressed carbonic acid; but so soon as this gas has reached the opposite side of the barrier, and there escapes, its elastic force being restrained by no compression, causes it to assume its original dimensions.

171. It will be readily perceived that the theory here given depends on the principle, that however much a gas is condensed, it will at all pressures rush into a vacuum with the same velocity. The elasticity of a gas in any state is measured by the force under which it exists, and this is ordinarily the pressure or weight of the atmosphere; it follows, therefore, that though the density of gases may vary, yet they have all the same elastic force; but, when pressure is exerted upon them, the density and elasticity increasing together, their velocity in rushing into a void is always, and under all pressures, a constant quantity.

172. We may now apply this reasoning to certain practical cases. Mr. Graham found that the absorption of carbonic acid by a porous plug of stucco was very small in amount, and the absorption of atmospheric air is equally minute. Accordingly, when these two gases are separated from each other by a screen of that substance, they diffuse according to the law of the square roots of their density. One volume of air, replacing 0·8091 of carbonic acid, the gas, therefore, on that side of the screen where

the carbonic acid was increases in quantity. Now when, instead of a screen of stucco, a thin lamina of India-rubber is used, which is found, upon trial, to condense one atmosphere of carbonic acid, while it does not act upon air, the same rate of exchange ensues; but there is a diminution of gaseous matter on the side containing the acid, and because the screen condenses one atmosphere, there should be found only half as much gas as would represent the equivalent volume of diffusion had the screen possessed no condensing power.

173. One hundred and sixty-one measures of carbonic acid gas were confined in a tube under a thin sheet of India-rubber, and suffered to diffuse for thirty-six hours. To prevent as much as possible any disturbing action of the fluid over which the experiment was tried, a saturated solution of common salt, which absorbs carbonic acid slowly, was made use of. The gaseous contents of the tube decreased in their dimensions very rapidly, and when measured, were found to consist of 98 volumes only. In the mean time, a tube closed at one end, filled with the same quantity of carbonic acid, and placed by the side of the former, had decreased about five measures; we may therefore assume that the quantity of gas that should have been found in the diffusion tube ought to have amounted to 100 measures nearly. Now the specific gravity of carbonic acid gas is 1·527, the reciprocal of the square root of which is 0·8091. Hence, under ordinary circumstances, one volume of air should replace 0·8091 of carbonic acid gas; but as, in the experiment here tried, the barrier produced a compression, one volume of air should displace 1·6182 of carbonic acid, the amount observed very nearly.

174. I would not here be understood to say that there are no other disturbing actions going on in cellular tissues except those which result from their absorbent power. A great many facts show that, under peculiar conditions, they are able to produce decompositions of a certain sort. Often their regular action, as indicated by theory, seems to be entirely departed from; great disturbance arising from the fact, that when two gases are absorbed together by any areolar tissue, they experience a greater condensation than each would in a separate state. The presence of nitrogen or carbonic acid, in any porous mass, increases the action of that mass on oxygen, more of the latter being condensed. A piece of charcoal, impregnated with oxygen, condenses more hydrogen than it should do, and the presence of hydrogen facilitates the condensation of nitrogen. It is, therefore, impossible to foretell what the result of diffusing one gas into another will be, by simply ascertaining how many volumes of either alone will be absorbed by the tissue, inasmuch as a greater or lesser condensation may happen when both are employed together.

175. Variations of temperature, which probably affect the power of absorption, and thereby the diffusion of volumes, are experienced by all tissues. When charcoal, or any other porous mass, is placed in an atmosphere of gas, which it can condense rapidly, its temperature rises, the effect apparently depending more on the velocity of absorption than on the final amount. In the case of ammonia, it does not even require a thermometer to discover this increase of temperature, for it is very sensible to the touch. On the other hand, when this condensed gas makes its escape, a corresponding diminution of temperature happens: it is immaterial by what means the liberation of the gas

is effected, the same result uniformly follows; if, for example, a porous mass saturated with carbonic acid be exposed to an air-pump vacuum, in connexion with a thermometric arrangement, the gas, as it is liberated from the pores of the structure by the action of the pneumatic machine, gives rise, by its expansion, to the production of cold. Or, if the same porous mass, saturated in like manner with carbonic acid, be exposed to an atmosphere of hydrogen, it absorbs but a small quantity of this latter substance, while a very large amount of the former is liberated from its condensed state, and the thermometer indicates a fall of temperature; the resulting volume of the mixed gases being much larger than the original volume of hydrogen. Again, if a porous mass, which has absorbed its due volume of hydrogen, be immersed in an atmosphere of ammonia, the resulting volume of the mixed gases is much smaller than the original amount, and the porous mass becomes hot.

176. The observations here made on the vicissitudes of temperature which an areolar mass experiences, when successively immersed in an atmosphere of different kinds, obviously apply when the exposures, instead of being consecutive, are simultaneous. If, for example, a barrier separates carbonic acid and hydrogen gas, and absorbs the former to a large amount, but exerts little or no action on the latter, then the opposite sides of that barrier will be unequally heated. Suppose, for illustration, we call that surface of the barrier which looks towards the carbonic acid, C, and the surface looking towards the hydrogen, H: then, because of the condensing action of the barrier on the acid gas, the surface C will become hot; but because the gas, so soon as it has passed the barrier, expands, as into a void, when it reaches the surface H that surface will become cold. We see, therefore, that immediately after the action of the membrane or barrier is first set up, the absorption of the carbonic acid takes place on a hot surface, and its evolution from a cold one; whereas the absorption of the hydrogen takes place on a cold surface, and its liberation from a hot one. A modified result of course happens when both gases are absorbed in different degrees, and any prediction of the resulting action becomes a matter of much difficulty. Where the barrier is very thin, or has a high conducting power as respects caloric, this distinct surface action may not rigidly occur, but the whole of the structure experiences some determinate rise or diminution; a mean of the condition of the two surfaces respectively.

177. The obvious application of these results, in a physiological point of view, is to the function of respiration. In no order of life, however, does the respiratory mechanism coincide with the arrangements of two gaseous media separated from each other by a barrier. In those tribes which breathe by lungs, the pulmonary vessels present themselves on the remote bronchial cells, and the arrangement is, in effect, a liquid and a gas, parted from each other by a membrane. Chemical physiologists have hastened to apply the discovery of Dr. Mitchell in this case, and have done right. But still the chain of evidence is incomplete, for we have not yet seen it proved that gaseous matter, in union with a liquid, will leave it and pass through a barrier to join a gas on the other side. The following experiment will supply this defect: A small jar, the mouth of which was closed with India-rubber, and its opposite end made to terminate a tube one eighth of an inch in diameter, while full of atmospheric air, was sunk in a vessel containing

water impregnated with carbonic acid: in a very short time, the acid gas, leaving the water, went through the barrier, and as it accumulated in the jar, was delivered by the short tube at the other end, and passed up in bubbles through the water.

178. Branchial respiration deviates still more from the simple type, for we have here two fluids, presenting gaseous matter to each other for interchange through a membranous screen. In one of them the gas is in a state of solution only, but as to what its condition in the other may be, we can scarcely say. The phenomenon, however, becomes obviously much more complex. In bronchial respiration, the account which Mr. Graham gives of the process by which the little cells empty themselves into the trachea is probably correct; and the same observation undoubtedly applies to the case of the respiration of insects.

179. The issue of these investigations, besides co-ordinating the observations of Dr. Mitchell and Professor Graham, has a far more important application. It shows us indisputably that membranes have special mechanical functions depending on the conditions of their texture; and that often they are, *in appearance*, the generators of power equal to the pressure of many atmospheres. It is not pretended, however, that the foregoing paragraphs contain the whole theory of cellular action, the object of this communication being limited to a discussion of some of those mechanical functions which have led chemists to conflicting results. Writers on physiology have suspected that membranes were springs of power, both mechanical and chemical, but the direct proof, from actual experiment, has never until now been furnished.

CHAPTER VII.

THE PHYSICAL THEORY OF ENDOSMOSIS.

(*From the American Journal of the Medical Sciences for August*, 1838.)

CONTENTS: *Relation of Endosmosis to Capillary Attraction.—Cases of reported Decompositions.—Can be produced by Inorganic Masses, and therefore not due to Vitality.—Water made to wet Mercury.—Voltaic Battery controls Capillary Attraction.—Action of Inorganic Tissues.—Water passes through excessively small Pores.—Hydraulic Currents.—Deposites produced by Endosmotic Currents.—Apparent Decomposition of Metallic Salts by Membranes.—True Theory of it.—General Conclusion that Endosmosis is nothing more than common Capillary Attraction, and never occasions true Decompositions.*

180. It is the object of this communication to offer some proofs that the peculiar force known to chemists and physiologists under the title of endosmose and exosmose, has no existence independently of ordinary capillary attraction; and that all the cases described as chemical decompositions, brought about by the intervention of animal membranes and areolar tissues, are only examples of the play of ordinary and well-known agents.

181. It is necessary, before entering into a critical examination of these points, to explain briefly the leading experiments which have been reported in connexion with the subject. Often received upon doubtful evidence, and sometimes implying the existence of laws which, if established, would compel us to modify our opinions of chemical agency in general, it is time that the whole of them should pass under a connected review, their bearings upon each other be properly designated, and the true value which ought to be attached to them ascertained. If thus examined, it will be found that they are very far from establishing the points supposed; and in compliance with the usages of science, their application must be rejected.

182. We shall have to consider, 1st, those experiments which refer to changes of hydrostatic level of liquids, and the production of mechanical results; 2dly, those which are reputed to be examples of chemical depositions brought about by tissue action.

83. The original experiments of Porrett, Fischer, and Dutrochet, are instances of the first class; from them the terms endosmose and exosmose are derived. They are essentially illustrations of the fact, that if two fluids be separated from one another by a porous barrier, they will mutually traverse it, but very often not with equal velocities, for the volume of the one passing in a given time may exceed the volume of the other, and hence a disturbance of their hydrostatic level results. If, for example, we take a tube, and close one of its ends with a piece of bladder, securely tied on, and fill it to a certain mark with alcohol, and then place it in a vessel of water, taking care that the hydrostatic level inside the tube, and that on the outside, shall coincide, in the course of a few hours it will be found that this equilibrium has been entirely disturbed, and the level of the alcohol risen. On reversing the arrangement, and placing the water in the tube and alcohol on the outside, there will be, at the completion of the experiment, a similar disturbance, but now the level will be found to have fallen.

"(*a.*) In this way it was found that there was endosmose from water to gum-water, to acetic acid, to nitric acid, and especially to hydrochloric acid; but that there was not endosmose from a liquid to itself.

"(*b.*) And that different animal and vegetable membranes enjoyed the same properties as bladder, in different degrees, and that plates of burned earth, or calcined slate, or clay, and, in general, all aluminous substances, possessed analogous powers, though to a much less extent.

"(*c.*) To explain these phenomena, it is necessary to resort to some force different from ordinary capillary attraction, or, at least, to some new modification of it; for the forces of capillarity, such as they are now understood, are totally insufficient to produce these results."—(*Pouillet.*)

184. The second class of experiments, though often affording well-marked illustrations of change of hydrostatic level, is chiefly important from the instances of apparent decomposition exhibited. From these it has been inferred, not only that membranes possessed certain definite chemical powers, but that at times they gave proof of a predilection for the passage of certain bodies in determinate directions through them.

185. If litmus water be placed on one side of a piece of bladder, and alcohol on the other, the water will forsake the colouring matter to pass through the bladder and unite with the alcohol.

186. If ferrocyanate of potassa be tied up in a section of intestine, and immersed in a solution of protosulphate of iron, Prussian blue will be deposited on one side of the intestine, but not on the other; hence it is inferred that one solution is suffered to pass through the pores, but a like passage is denied to the other.

187. If a solution of oxalic acid be placed on one side of a membrane, and lime-water on the other, clouds of the insoluble oxalate of lime will form on the side of the lime-water, but the other side will be pellucid.

188. If a volume of nitrogen gas in a soap-bubble, or under any suitable membrane, be exposed to atmospheric air, decomposition of that air will result, its oxygen passing through the membrane to form atmospheric air with the nitrogen within.

189. If a quantity of commercial alcohol be tied up in a bladder, and freely exposed to the air, the water in union with that alcohol will pass through the pores of the bladder, and, gradually evaporating away, will leave the alcohol much stronger.

190. And, lastly, which is by far the most remarkable of these phenomena, if a tube, the extremity of which is closed with membrane, be filled to a certain height with distilled water, and there be placed in it a few iron nails, on adjusting it hydrostatically, and suffering it to remain for a time in a solution of sulphate of copper, the membrane will apparently decompose the solution of the metallic salt, the base of which, in a deoxidized state, will remain attached to the under side of the membrane, but the acid and oxygen will traverse it, and be removed by uniting with the iron.

191. The body of evidence here furnished would go to show that membranes possess remarkable habitudes with respect to liquids, and, accordingly, it has been brought forward as the foundation of many physiological hypotheses. Nay, more, from hence it has been assumed that these were in truth nothing more than manifestations of that principle of vitality which is supposed to be the result of organization. A power, known under the name of endosmose, distinct from all other known agents, has been created, its especial office being to bring about certain molecular changes, in a way resembling outwardly, but essentially differing, from those of chemical affinity.

192. The error of this position might readily have been detected. We surely should not regard that as a specific force of vitality which is possessed by inorganic matter; yet, in the outset of the original experiments on the subject, it was found that alumina exhibited the same action as bladder, though in a feebler degree. As to the amount of force, with that we have nothing to do; for, no matter in how small a degree soever it may be that alumina possesses this character, the mere fact of possessing it at all goes to show that it is not a consequence of organization, or an evidence that the substance exhibiting it has ever been moulded by the powers of life.

193. The verification of Dutrochet's experiment with alumina becomes, therefore, a matter of the greatest importance; its extension to other inorganic substances would decide the point, and separate at once the power by which infiltrations take place from the powers of vitality. It has, however, been stated that those minerals in which this property has been observed possess it in a low degree. Some chemists have extended this observation, and class with alumina other bodies of a porous texture, as certain varieties of slate. But many experiments that have been made on this point have led to er-

roneous results, through inattention to the conditions of hydrostatic equilibrium. If two fluids be placed in the opposite arms of an inverted siphon, they will have a common level only when their specific gravity coincides; and, under all other circumstances, the height of their columns will be inversely proportional to their specific gravity respectively. If, then, we take a tube, and make its extremity end in a fine capillary termination, or close it with a plug of wood or of stucco, and fill it with some dense solution, such as chloride of sodium, sulphate of potash, or sulphate of copper, and then immerse it to the *same* level in pure water, the level of the fluid in the tube will descend in obedience to the laws of hydrostatics, and when a position of equilibrium is gained, the heights of the fluid inside and outside of the tube will be inversely proportional to their specific gravities. In all this, endosmosis or capillary attraction has no concern.

194. In structures the pores of which are of such a diameter that these adjustments of level can freely take place, the mechanical phenomena of endosmose are not visible; there is no fact that can indicate what is the true action of the porous body. All bodies which exhibit these phenomena have their pores of such a size that, while they offer resistance to change of level by mere leakage, they allow indefinitely small colums of the fluid they are exposed to to interchange through them.

195. We are not to expect that any of the phenomena of molecular infiltration will be exhibited when the apertures through which transudation occurs are of considerable size. If a piece of coarse linen is made use of as the separator of two fluids, those fluids will commingle without any disturbance of hydrostatic level. Whenever this latter equilibrium can be effected, it takes place, and entirely masks the molecular action of the mass. For this reason, most minerals fail to show the change of level when water passes into alcohol. They do not possess the exact kind of porosity required, either having their interstices of so large a size that derangements of level can be quickly compensated, or, on the other hand, being totally impervious to the liquids. I found that the common white earthenware, when its glaze was removed, allowed water to percolate through it to gum-water, but no disturbance of level was observed, simply because the freedom of communication between the two liquids was so great, that if one of them had a higher level than the other purposely given to it, it soon returned to its original position of equilibrium. A fragment of thick Hessian crucible gave the same result, as also several varieties of slate, iron slate and mica slate, some of which were calcined and others in their ordinary condition; also a fragment of common writing slate, which had undergone semi-vitrification in the forge. This freedom of communication was noticed in some specimens of soapstone, both burned and unburned; and while, in plaster of Paris, the experiment failed because the apertures were too large, in the transparent micas it failed for want of communication. To show, therefore, the original experiment of Dutrochet, the interstices of the barrier must be of such a diameter that all mechanical compensations for change of level are hindered, and free molecular infiltration can take place.

196. If a tube half an inch in diameter and three or four inches long be sealed at one end, and while the glass is yet warm be dipped into water, a number of small cracks will be made in its bottom. This forms a very useful instrument for studying the properties here under discussion. If it be filled to a certain mark with alcohol, and

then plunged to the same level in water, an apparent endosmosis through the cracks is the result, for the alcohol rises with considerable velocity in the tube. It is, however, only an apparent endosmosis, for, upon closer examination, it will be found that the motion stops when the hydrostatic equilibrium is adjusted through the chink (193). Any of the very porous minerals show the same thing.

197. If, in the last experiment, the tube be filled with lime-water, and then immersed to the same level in a solution of oxalic acid, the appearance described in (187) will be reproduced. This cautions us not to impute to membranes any predilection for passage in certain directions; for that may arise from extraneous circumstances, and in the instance referred to, as will presently be shown, originates in a very different cause.

198. The relation existing between solids and fluids which determines their descent or rise in capillary tubes, has been referred to heretofore in these papers (123). A connexion of the phenomena of endosmosis and capillary attraction might have been traced to the fact that no liquid will pass through a barrier the surface of the pores of which it cannot wet. The relation of glass and quicksilver to each other, in this point of view, is interesting. When a piece of glass is laid upon the surface of this fluid metal, contact between them does not take place, but they are separated from each other by an exceedingly small interval. As I had failed in reproducing many of the results attempted by means of artificial chinks in glass (196), because of their magnitude, I was led to hope that better success would attend the same attempts by making use of the small interstice between glass and mercury. A tube half an inch in diameter was therefore taken, and one of its extremities, having been ground truly flat, had its roughness taken off by exposing it carefully to the blowpipe flame. When the tube was lowered with this extremity downward, on the surface of some pure mercury, all the parts of its circumference touched the metal at once. A solution of green vitriol was placed in it to a certain height, and upon the mercury; on the outside of it was poured a solution of ferrocyanate of potassa. It was expected that, through the chink between the mercury and the glass, the liquids would slowly infiltrate to each other. After several days, no such action was observed, and the experiment, though repeated under a variety of conditions, afforded no better result. Now water and saline solutions, as will hereafter be shown, pass through interstices much more minute than this can reasonably be supposed to be; it is evident, therefore, that the want of action is mainly due to the circumstance that water and saline solutions generally do not wet mercury, and the laws of capillary action would indicate that, under these circumstances, they would fail to pass through the chink.

199. The event of this experiment points out, in an impressive manner, the general relations that must exist between the solids and liquids of organized bodies. Water will pass with great rapidity through a chink the width of which is not more than the half of a millionth part of an inch, provided it can wet both sides of that chink; but if this condition be not fulfilled it fails to pass, even though the width should increase to upward of one hundred and forty-four times its former dimensions.

200. That the non-passage described is here referred to the true cause, will appear from the following experiments. As has been stated, under ordinary circumstances,

water does not wet the surface of pure quicksilver, but stands upon it in drops of a more or less rounded form: if, however, the electrical relation of these substances be changed—if the mercury be in contact with the negative pole of a voltaic battery, and the water with the positive, a remarkable phenomenon ensues—the water now wets the mercury. To this important fact, and its applications, I shall hereafter return.

201. The best method of showing that the voltaic battery has entire control over capillary attraction is to take a shallow vessel containing a quantity of mercury, as A A, *fig.* 23, and place upon it, in the position marked *a*, a drop of water; on making this drop communicate with the positive electrode of a battery, and the mercury with the negative, in a moment the drop loses its rounded form and spreads out in a thin sheet on the metallic surface, completely wetting it, and according as the tension of the battery increases, the drop expands more and more. Thus, if the current from 5, 10, 20, 40, 80, &c., plates be successively passed through it, the diameter of the circular space it occupies closely follows the increase, and appears to continue to do so until the drop becomes so thin that the electricity, in the shape of a spark, can pass through: then, of course, the experiment cannot be continued.

202. If, therefore, in the arrangement of (198), the electrical relations of the saline solution and the mercury be changed, by the process here indicated, we should expect that the passage through the chink would take place; and it is so. This experiment affords a very elegant illustration of a result obtained by Porrett many years ago, which was applied by Dutrochet to the explanation of endosmose. He observed that if two quantities of water were separated from each other by a membranous partition, and one of them made positive and the other negative, all the water in contact with the positive pole would escape through the membrane into this negative partition. When, in the arrangement of (198), the water escapes through the chink on being electrified, it does not move slowly, by the action of its own weight, but is also impelled downward in the way described by Porrett. For, take a tube, *c c*, *fig.* 24, the diameter of which is about one tenth or one twelfth of an inch, and insert in its axis a platina wire, *a*, then let the lower extremity touch a surface of water, and a volume of that fluid will rise in it to a certain height by common capillary attraction. If, now, the tube, charged with its water and wire, be placed, as in the figure, on the surface of some mercury in a watch-glass, so that the extremity of the tube shall just touch the metallic surface, and the wire *a* be then connected with the positive electrode, and the wire *b* with the negative, in an instant the water will begin to flow out of the tube, and spread over the mercury, and will continue to do so until its level has sunk to the end of the platina wire. With a wider tube, such as that described in (198), this passage might be imputed to the mere gravitation of the parts of the fluid urging them downward; but, in this instance, owing to the narrowness of the tube, that force is nullified by capillary attraction. The water is therefore driven out of the tube by an active force; and that this is really the case, is abundantly proved by breaking the battery connexion and raising the tube slightly above the mercurial surface; the water then precipitately returns back into the tube again.

203. In Chapter V. it was shown how the common phenomena of capillary attrac-

tion originate in electrical excitement. The fact that electricity has, therefore, an entire control over the motions of fluids in capillary tubes, will not be at all surprising. This leads us to a generic resemblance between the phenomena of chemical affinity and those of capillarity, which deserves a much more detailed investigation. Treatises on chemistry represent a number of disturbing agencies which frequently antagonize, and often control the operations of affinity; these are cohesion, elasticity, quantity of matter, gravity, and the agency of the imponderables. It is, however, a mistake to enumerate either quantity of matter or gravity as ever disturbing the action of affinity. Gravity, it is true, may cause the lower parts of a solution or an alloy to be denser than the upper; but an action of this kind is not to be accounted as an example of contrariety in the forces. It does not exhibit them at all at variance with each other, or in any manner neutralizing each other. Similar remarks might be made in respect to quantity of matter. The elastic state, being merely a condition of cohesion, influences the action of affinity by presenting bodies under a modified form as respects their cohesion. Strictly speaking, there are but two forces which in reality control affinity; these are cohesion and the agency of the imponderables. And these are the forces that control capillary action, the phenomena of which are the results of the equilibrium of an attractive force on the one hand, and cohesion on the other. They may be regarded as modified cases of chemical affinity, and, being brought about by the operation of the same forces, are under the control of the same disturbing agents.

204. In (195) I have enumerated different cases of ineffectual attempts to recognise the action of endosmosis in inorganic bodies. I have also shown the peculiar disturbance that arises when interstitial communication is too free, and the relation that must exist between a solid and a fluid for molecular transudation to happen. Now, when all these conditions are fulfilled in any barrier, the phenomena of endosmosis will take place irrespective of its nature, whether it be organic or inorganic. Plates of kaolin or porcelain clay from Villarica, disks of steatite from Brazil, after undergoing induration in the fire, and a variety of compact sandstone being cemented on the end of a tube, exhibited in a very satisfactory manner the passage of water into gum-water, even against hydrostatic pressures of several inches. Independent of this decisive evidence, it might be determined that an organized tissue is not essential to this process, from the circumstance that common writing paper, fastened with sealing wax on the end of an open tube, exhibits the endosmosis of water into gum-water in a much more striking manner than bladder; and certain inspissated juices of plants, as caoutchouc, when in thin layers, act very well, though not so rapidly. Of all substances hitherto tried, filtering paper, imbued with coagulated albumen, acts most satisfactorily.

205. A repetition of these experiments, made under a variety of circumstances, leaves no farther doubt as to the true character of Dutrochet's endosmosis. It is not, as some would have us suppose, a vital or a semi-vital force; it is nothing more than a peculiar case of capillary action.

206. The conditions which this peculiar case requires are, that both the fluids shall be able to wet the barrier, that in a capillary tube formed of it they should rise to different heights, and that they should be able to unite chemically with each other. If we

suppose a tube of such a length, with respect to its diameter, that a fluid in which it is immersed shall rise to the top of it, and that some extraneous cause shall effect its removal as fast as it reaches that position, it is evident that a continuous current will traverse the tube. A case in point is the action of the wick of a lamp, along which the oil continually ascends, because it is removed by chemical decomposition as fast as it reaches the highest point of the capillary system of cotton fibres. Any other cause which would effect its removal in as complete a manner, would equally produce a continuous current. The same explanation applies in the case of water passing through a tissue of bladder to alcohol; for as soon as the small columns which percolate through that tissue meet with the alcohol, they are removed by uniting chemically with it, and a continuous current, therefore, results. The current of alcohol that takes place in the opposite way meets with a similar fate; and the excess of one of these currents over the other determines in what direction the hydrostatic level shall change. Even the reputed decompositions brought about by endosmosis are not without very homely and well-known analogues. The greasy wick, when dipped into a lamp containing oil and water, removes the former without disturbing the latter.

207. It has been shown that, to exhibit the phenomena of endosmosis, pores of a certain size are necessary; that if their diameter exceed this, the mere leakage masks every other effect. We might next proceed to investigate what are the actual dimensions demanded. This inquiry is not alone one of mere curiosity, but meets with important applications in every department of physiology; and the problem, if successfully solved, would cast a great deal of light on the interstitial communications that take place in every part of organic structure. Vessels of an excessive degree of minuteness creep through the finest tissues, which might almost be regarded as formed by the interlacings of these narrow capillary tubes. The immediate apertures of communication between the remote fibrils of the artery, the vein, and the duct of any gland, are of an indescribable smallness; yet, how great a share of the aggregate of the actions of life is carried on in such little pores, which are too small for the injection of the anatomist to reach, or even for microscopic vision to descry.

208. These pores are, however, capable of approximative admeasurement; or, at least, their dimensions may be determined within limits of error. The method by which this can be accomplished essentially depends on the circumstance, that if any fluid will *wet* two or more solids, it will rise in capillary pipes formed of them identically to the same height, no matter what their chemical constitution may be, provided their diameter is the same. Thus, water will rise in a tube of glass, of serous membrane, or in a straw, to the same height, if the diameters be alike.

209. It has been stated (199) that water will pass into a chink the width of which is not more than the half of a millionth part of an inch, under the condition that it can wet both faces of the chink. Sir I. NEWTON has shown (*Optics*, b. ii., p. i.) that if you lay a convex lens of long focus on a glass plane, a series of coloured rings surrounding a central black spot will emerge; and it is known from simple geometrical principles, that the greatest distance between the two glasses, in any part where the black spot appears, does not exceed the half of a millionth part of an inch. Yet, if a drop of water be

placed between the glasses, it will be perceived to make its way rapidly to the central spot, certain optical changes, depending on its superior refractive power as compared with atmospheric air, accompanying its progress; and hence we infer, that if a chink or cleft, not exceeding the half of a millionth of an inch, occurred in an animal tissue, water would find its way into it.

210. In vessels of large diameter, fluids readily adjust themselves hydrostatically, and currents set in any direction without obstruction. When the dimensions of the containing vessels become very small, a new order of things is set up, and the particles have, as it were, to obey newly-created forces. In large masses, the action of gravity produces the leading phenomena, and the effects of all the molecular forces vanish. When minute quantities are operated on, the action of gravity diminishes, and friction, cohesion, capillary action, and other molecular forces, become obvious. The mechanical relations, therefore, of small and large quantities are totally distinct: hydrostatic equilibrium, which is effected so readily in larger vessels, is accomplished with more difficulty through pores, and as these decrease in their dimensions, the forces of resistance rapidly increase. Water, at all pressures, will adjust itself hydrostatically with great readiness, when it is obstructed by a porous medium, provided the pores are of sensible size; but if that size diminishes, the resisting forces continually increasing, the conditions of hydrostatic equilibrium are fulfilled with more difficulty, and at last cease to be fulfilled at all.

211. The foregoing remarks enable us to come to a decision in reference to the character of endosmosis, as indicated in (183). We perceive that this force, far from being the attribute of organized matter, exhibits its phenomena when substances whose inorganic character is unquestionable are made use of. A variety of porous minerals may be employed in lieu of organic tissues with success; and, if the endosmosis of gases be allowed to be a phenomenon of the same kind, then we know that such liquids as water may be employed as barriers; a peculiar degree of porosity is required, a structure dense enough to obstruct readily hydraulic currents, but open enough to allow very small columns of fluid material to traverse it. A crack in glass, because of its width, allows too great a freedom of motion; but bladder, peritoneum, or condensed cellular tissue, fulfil, at ordinary pressures, the required condition. It is necessary, too, that the liquids under trial shall wet the surface of the solid; for want of this action, water fails to pass through the narrow interstice between mercury and glass. The degree of pressure generated either during the action, or existing at the commencement of the experiment, is an important element, for upon it depends the appearance or non-appearance of the phenomenon. Thus, at ordinary pressures, bladder will exhibit the change of level when water passes into alcohol; but if pressure on one side of the membrane be increased, a hydraulic current sets through it, and the experiment fails, because the success of the result depends on the excess of the molecular over the hydraulic current, and in this case the latter predominates. The relation which the liquids bear to each other is also important; the facility with which they unite with each other, and therefore remove each other on transuding through the barrier, may sometimes make up for an increased size of the pores.

212. There is no absolute diameter at which a pore will cease to permit a hydraulic current to pass it, and the phenomena of endosmosis to commence. Size is but one of the elements involved in producing this action, and deviation in respect of it may be often compensated by variations in the other conditions; its relation to pressure is not unimportant. Water has been forced through the interstices of gold, and melted tin through the pores of solid copper.

213. We have next to consider the different cases of decomposition, ostensibly brought about by endosmosis.

214. If litmus water be placed on one side of a piece of bladder, and alcohol on the other, the water will forsake the colouring matter to pass through the bladder and unite with the alcohol! This experiment, which was originally cited by me as explanatory of the fact, that colouring matter in the intestines could not give its peculiar tint to the chyle, does away with one of the most important objections to the direct absorption of medicaments by the lacteal system. In estimating its true value among the facts now under consideration, we shall find that it is very far from supporting the hypothesis that chemical decompositions can be brought about by endosmosis. There is no proof that the colouring matter, though permanently suspended in the water, is chemically united with it; analogies would lead us to the very opposite opinion. All that can be predicated of this experiment is, that it exhibits a refined kind of filtration, which, probably, may hereafter become of considerable importance in its applications in the arts; as in the separation of colouring matter from solutions, or the preparation of medicines, such as the vegetable alkalies, which should be formed from colourless solutions. It is probable that the non-solubility of litmus in alcohol is not without its influence in this matter.

215. The results referred to in (186), (187), (188), may all be classed together. They have been taken as proving that currents may set in determinate directions through a membrane; thus, it has been inferred from the experiment (187), that when a solution of oxalic acid is on one side of a membrane, and lime-water on the other, the acid passes freely through the pores, but a passage to the lime-water is denied. It has been thought that an action of this kind was the result of organization, an important property possessed by membranes only; hence it has been inferred that tissues allowed of the transit of bodies in certain directions through them.

216. A more careful investigation of the circumstances deprives this phenomenon of all its mysterious importance. It is by no means confined to tissues or organized matter (197). If we take a cupping-glass, the edge of which is truly ground, and, having filled it with lime-water, place it upon a piece of clean plate glass, then, on pouring a solution of oxalic acid on the plate, so that it may encircle the edge of the cupping-glass, it will be perceived that, while the acid solution on the outside remains clear and colourless, innumerable streams of oxalate of lime will pass from the bottom of the glass, and rising in white clouds, render the solution turbid. Here, surely, we cannot ascribe any organic function to the chink between the two pieces of glass; yet the current apparently sets only in one way, and exhibits, to all intents and purposes, a phenomenon identically the same with that referred to in (187).

217. The instances here referred to are those which have hitherto attracted attention.

Some physicians have made important, but unwarrantable deductions from them. Sacs formed of animal tissues have been supposed to be competent to expel saline matters from them by exosmose, while they were introducing water by endosmose. No general rule of this kind will apply, nor will the hypothesis that a current passes in one direction only, bear the test of a close examination. It has been stated that, when oxalic acid and lime-water are separated from each other by a membrane, a precipitate of oxalate of lime takes place on the side of the lime-water, and, therefore, the current sets from the acid to the lime-water, but none in the opposite direction. The same occurs in the case of Prussian blue, which is always found on one side of the membrane. The action is correctly reported, but the inference is erroneous.

218. I took a number of tubes, open at both ends, and tied a piece of bladder on each. They served to contain a fluid which might communicate with one of a different kind, capable of giving a precipitate, contained in a glass receiver.

a Contained sulphocyanate of potassa in the tube, and solution of persulphate of iron in the reservoir.

b Contained solution of iodide of potassium in the tube, and solution of bichloride of mercury in the reservoir.

c Contained solution of oxalic acid in the tube, and lime-water in the reservoir.

d Contained solution of chloride of barium in the tube, and dilute sulphuric acid in the reservoir.

e Contained prussiate of potassa in the tube, and persulphate of iron in the reservoir.

f Contained prussiate of potassa in the tube, and protosulphate of iron in the reservoir.

g Contained bichromate of potassa in the tube, and acetate of lead in the reservoir.

h Contained dilute muriatic acid in the tube, and solution of nitrate of silver in the reservoir.

i Contained solution of prussiate of potassa in the tube, and sulphate of copper in the reservoir.

219. In the course of a few days it was found that, in the arrangement marked *a*, *c*, *d*, *f*, the level within the tube had risen, and the deposite of sulphocyanate of iron, oxalate of lime, sulphate of baryta, and Prussian blue, had taken place within the tube; in the arrangements *b* and *g*, the precipitates of biniodide of mercury and chromate of lead were entirely interstitial, the pores and cellular tissue of the membrane being choked with them, but none had escaped into the fluids on either side of the barrier; the membranes, when thus injected, formed very pretty microscopical objects; the level in the tube *b* had risen, but that in *g* had fallen. In the arrangements *e* and *h*, the level in the tube had risen, but the deposite was found on the outside. In *i*, after several days, no action of any kind could be perceived, due, perhaps, to the unusual thickness of the membrane.

220. From the aggregate of these experiments we gather, that in nearly all cases where two fluids, which, being mixed, give rise to an insoluble precipitate, are separated from each other by a membrane, the precipitate will be found on one or other of the sides of that membrane, but hardly ever on both; sometimes the action appears to be checked by the choking of the pores and interstices, and then little or no deposite is found on either side of the tissue.

221. In giving an explanation of these curious facts, it is to be borne in mind, that all the phenomena treated of in this chapter are the results of two contemporaneous currents, endosmosis never existing without exosmose. If these currents are established in fluids, which, by their union, give rise to solid matter, its deposite may occur under all the forms designated (219), (220). If, for instance, the precipitate be a light material, and one of the currents exceed the other in volume, the small particles, as they are formed, are drifted by the current, which is acting under the greater advantage, and the deposite will take place wholly on one side. This, I suppose, is the mode of deposite of oxalate of lime, which always goes with the greater current. The chemical change, or union, it is to be remembered, takes place at the point of contact of the two fluids, which is necessarily in the membrane itself; there they neutralize one another, and if the circumstances of the experiment permit, the more powerful current carries before it the precipitating particles as fast as they form, and the excess of unneutralized material in it produces a precipitate of the same kind as soon as it mingles with the mass of fluid on the side of the membrane towards which it is going. But, if any disturbing causes intervene, if the precipitated matter has any affinity for the fibre of the membrane, in the manner of a dye, or if it be too bulky to pass through the pores, or too ponderous for the current readily to move, it is detained on the spot where it was generated, and in a very short time the tissue becomes choked; the biniodide of mercury is subject to these circumstances. A number of disturbing causes will often change the results of these experiments: when, for example, the precipitating particles have a high density, their weight may carry them in a direction even opposed to the stronger current. The relative specific gravity of the two fluids may also determine the course in which the particles shall go.

222. That these experiments do not prove that membranes have a predilection for passage in certain directions through them, the results of the earlier writers are sufficient to show. They have shown that when Prussian blue is deposited in this way, sometimes the precipitate is towards the salt of iron, and sometimes towards the prussiate of potash, the direction it takes being often influenced by very slight causes. Nay, even analogous actions may be exhibited without using any membrane, barrier, or obstruction; if into a half-ounce vial a quantity of strong sulphuric acid is poured, and upon that a solution of chloride of calcium, so that the two fluids may intermix as little as possible, in the course of a few days it will be seen that, as the fluids slowly diffuse into each other, the sulphate of lime is deposited entirely in the supernatant solution, and none in the strong acid, a result unquestionably depending upon their relative specific gravity and cohesion.

223. Sections (188), (189), contain examples of what has been termed in these papers decompositions of a certain sort. In relation to the first of these, it is by no means clearly proved that oxygen really does leave the nitrogen in the atmosphere to go through the barrier. The two gases may respectively pass into each other, as atmospheric air and nitrogen. It is true that the more probable mode of passage is that assumed in the section quoted. If so, it does not even follow that a real chemical decomposition happens, for there is much reason to doubt whether atmospheric air is

itself a compound. The same observation applies, to a certain extent, to the result (189), which is an effect analogous to that produced by a few fibres of greasy cotton when dipped into a mixture of oil and water, as in a common lamp: effects which are totally distinct from chemical decomposition.

224. The experiment referred to in (190) is apparently the most important of these cases of tissue action. "A tube of suitable dimensions, having one of its extremities closed with a piece of bladder, *a b*, *fig.* 25, and filled to a certain height, *e e*, with pure water, into which a few iron nails were dropped, was immersed in a solution of sulphate of copper. In the course of a few days a deposite of metallic copper was found on the surface of the membrane towards *c*, but none on the inner side, or upon the iron nails. A number of other metallic salts, such as acetate of lead, nitrate of silver, &c., afford similar results."

225. Certainly this is a remarkable phenomenon, if the conditions under which it occurs are accurately detailed. The operation and laws of chemical affinity would lead us to ascribe the decomposition of sulphate of copper to the action of the iron; but then it would appear that those same laws require the metallic precipitate to be deposited on the disturbing metal; here, however, it is found that the deposite really occurs on the membranous tissue.

226. On placing on one side of a membrane pure water, and on the other sulphate of copper, or any other salt reputed to be capable of decomposition under the circumstances given, no chemical action whatever occurred, in many days, at all analogous to the phenomena; but the water passed out of the tube into the copper solution with considerable rapidity.

227. On repeating the experiment, as given in (224), in the course of six days the results were as follows: A great disturbance of hydrostatic level had occurred, there being an accumulation in the tube containing the iron nails; the under side of the bladder was coated to a considerable thickness with bright metallic copper, and a small portion, in a pulverulent form, was found on the inner edge, and here and there patches were discovered coating the surface of the iron nails, and minute veins of copper passing through the bladder. In many repetitions of this, the same results were uniformly observed.

228. These numerous experiments showed that in all cases there was a perfect metallic communication from the iron nails, by means of veins of copper, through the bladder, with the cupreous mass on the under side. A piece of zinc, *z*, was therefore suspended in the tube, *a a*, *fig.* 26, in pure water, at a distance of three quarters of an inch above the bladder, and the arrangement exposed to a solution of acetate of lead. For several hours no chemical action whatever occurred, the water exosmosing towards the metallic solution, and the level in the tube, of course, falling. At the same time, a small portion of solution of acetate of lead passed into the pure water, as was shown, after the lapse of twenty hours, by the formation of some thin filaments of lead on the lower edge of the zinc; these kept increasing in length, and finally reached the bladder; soon after, they appeared to have made their way through the pores of that structure, and then the usual deposite occurred on the under side of the mem-

brane: simultaneously, the hydraulic current changed, and the level of the fluid in the tube began to rise.

229. A semicircular piece of very thin sheet-iron was placed on the inside of the bladder; it caused, in a very short time, a copious precipitate of the copper on that part of the bladder on which it lay, the remainder being free from metallic deposite. A very well-marked change of hydrostatic level occurred. On leaning the tube, the thin piece of sheet-iron was found to adhere to the bladder; and, on tearing away the cupreous deposite, the little veins, communicating through the porous texture, were visible without a lens.

230. Lastly, when the relative position of the arrangement was reversed, the pieces of iron being placed below the membrane, and the copper solution above, so that the little filaments of metallic matter could not reach the tissue on account of their weight, but fell down; or when they were destroyed by mechanical means as fast as they formed, no deposite ever occurred on the bladder.

231. The experiments (228) and (230) especially, and the other experiments incidentally, prove that a perfect metallic communication must extend from the decomposing metal to the under side of the membrane, through the very substance of that structure; and this observation enables us to give the true theory of the process. Let *a a a a* (*fig.* 27) be a tube, divided in some portion of its length by a membranous partition, *b b*, on one side of which, A, pure water is placed, containing the decomposing metal, *c*, and on the other side B, the metallic solution intended to be decomposed. Owing to the obstruction caused by the membranous diaphragm, the diffusion of the water into the solution, and of the solution into the water, will be retarded; in the course of time, however, it will take place. Then, as soon as any of the metallic salt has reached the lower part of the decomposing metal, reduction ensues in virtue of the common play of chemical affinity, the first portion of the metal thus eliminated adhering to the surface of the decomposing metal, and forming with it an active voltaic couple; the decomposing metal being thereby rendered positive, and the eliminated metal negative. Of course, the reduction of the dissolved metal continues, the aggregate of filaments extending itself down by its weight towards the diaphragm; and as the solution becomes stronger and stronger the nearer the filaments approach the bladder, so the metallic deposite becomes more abundant. No obstruction is experienced in passing through the pores of the diaphragm, because the metallic solution is still present there, and is reduced. But the moment any one of the metallic veins has penetrated to the under side of the membrane, the process goes on with tenfold activity. The metallic deposite, when it first started from the decomposing metal, was, perhaps, merely a capillary thread, for the solution through which it was passing was so weak that a greater quantity could not be presented; as it neared the upper surface of the diaphragm, it became of stouter substance, because the solution, being stronger, the supply was more abundant; and now, having passed the barrier and reached the strong metallic solution, the deposite is at once at a maximum. Hence we account for the circumstance that the reduction of the metal chiefly takes place on the under side of the membrane.

232. It may here be asked, How is the acid in the metallic solution conveyed from a

distance to the decomposing metal, so as to unite with it? It was to encounter this query, and to answer it, that I have been induced to examine so much in detail the whole of this process. There is no physical difference between the deposites here spoken of, and those known formerly to chemists under the name of Arbor Saturni and Arbor Dianæ; for the bladder or other membrane being freely penetrated, impresses no sort of action or change on the process in any wise. The same difficulty here met with occurs in all such cases of metallic precipitations. For example, in making the lead tree, by the action of a piece of zinc on some saturnine solution, the particles of lead are often evolved at a distance of six or eight inches from the decomposing metal. How, it may be asked, is the acid transported in this case? More than a century ago, La Condamine endeavoured to account for this, but the state of chemical knowledge at that time did not enable him to assign the true reason, and he was forced to infer that currents actually traversed the solution, making their way to the decomposing metal. All decompositions brought about by the agency of the voltaic battery are cases in point. The elements of water are separately evolved when the electrodes are many inches apart, and the decomposition of the very salts here under consideration occurs in the same way. It is now generally admitted that these decompositions do not occur to solitary particles at a time; but a chain of them, extending from one electrode to the other, undergoes simultaneous decompositions and recompositions, a process which eventuates in the elimination of the separate ingredients at opposite extremities of the chain. This explanation applies to the case here considered.

233. In support of the explanation here given, the phenomenon of change of hydrostatic level affords a very powerful evidence. It was stated (228) that, previous to the establishment of a voltaic current from one side of the bladder to the other, the hydraulic current set from the water towards the solution, and, therefore, the level in the tube kept falling; but so soon as the voltaic current passed through the barrier, the hydraulic current changed, and the level in the tube rose rapidly. Similar changes of level have been noticed, when voltaic currents pass through membranes, by Mr. Porrett and other chemists; they appear to be certain indexes of that molecular interchange (not current) which eventuates in polar decomposition.

234. I therefore reject entirely this experiment, as having anything whatever to do with the phenomena of tissue action, or as affording an example of the powerful decomposing agency of membranes. It has been considered in detail, inasmuch as it is as necessary, in studying the elements of a science which, like physiology, is yet in its infancy, to discard whatever is irrelevant or erroneous, as to point out what is pertinent and true.

235. After a very full examination of the question before us, it is not to be concealed that the decision to which we are constrained to come is entirely unfavourable to the opinion commonly held as respects endosmosis. To rank it as a peculiar power is unquestionably erroneous; to regard it as a manifestation of organization, or the attribute of organized structures, is equally so. Ever since the leading phenomena were made known by Porrett, Fischer, Gustave Magnus, and Dutrochet, they have been extensively applied in the elucidation of kindred actions in the vital frame; the causes of a variety

of diseases have been explained, as also the rationale of some modes of treatment. A premature application of principles ill understood is to be deprecated. On the discovery of a new fact, it is not always easy to determine its relation and position in the chain of knowledge; a confused idea is generally entertained of it; nor is it until time and experience have abstracted from it whatever was mysterious and doubtful, that we see clearly its true locality in respect of other facts, how they bear upon it, and how it bears upon them.

236. We conclude, therefore, that endosmosis is not a new power, nor does it bear any peculiar relation to organization; but that it is a manifestation of capillary attraction.

237. That, so far as the examination in this memoir has extended, there is no case upon record in which endosmosis has effected a real and undoubted chemical decomposition; that several cases of such reputed change depend plainly on the action of other agents; and hence, that those reported instances of the production of secreted fluids by dead membranes, through this power, are fanciful illusions.

238. The experiments here considered are such as were conceived to be the most important, and those generally referred to, as substantiating conclusions which conflict with that here given. If others be known not subject to these or similar strictures, I am ignorant of them; such may, by chance, hereafter be found; the decision now given refers to what have been regarded as actual proofs, and not to contingent evidence. That chemical changes of all kinds occur in tissues and glands, is not to be doubted; but we must not confound together a change effected *in* a tissue, and one effected *through* it. Urine is readily separated from arterial blood *in* the kidney; yet would any one expect, on placing blood *upon* a kidney, that urine would drop through it? A candid examination of many of the fashionable applications of endosmosis to physiological functions will discover no wide difference between them and this hypothetical case.

239. It is well known that those who first cultivated this department of science, viewed it as a case of electrical action. In this they did not go far astray; the machinery being erroneous, though the principle was true. Capillarity is unquestionably an electro-statical phenomenon, and hence will hereafter come to be intimately allied with chemistry. An important and extensive series of effects, which now pass as instances of electrical attraction, will be assigned to it; such as the adhesion of colouring matter and dyes to cloth, the silvering to a looking-glass, the solution of salts, and generally all cases of union where the uniting bodies lose none of their prominent characteristics.

CHAPTER VIII.

ON THE USE OF A SECONDARY WIRE AS A MEASURE OF THE RELATIVE TENSION OF ELECTRIC CURRENTS.

(From the London and Edinburgh Philosophical Magazine for October and November, 1839.)

240. It is the object of this memoir to establish the following propositions:

1st. That by means of a secondary wire, we may always determine the relative tension of electric currents.

2d. That there is reason to doubt whether the processes usually supposed to affect the condition of an *electric current* are ever attended with any such result; but that, when changes have apparently taken place, it is probable that they may be directly traced either to a disturbance at the place of generation, or to the development of other currents of a different character, the primary current itself remaining unchanged.

3d. That there are two different methods of accomplishing these disturbances, and thereby of raising the elastic force of a current: 1st. That tension may be augmented by the sacrifice of quantity; Volta's plan of a reduplicated series, and Henry's riband coil, in its condition of equilibrium, being examples: 2d. By the introduction of new affinities in the exciting cells; batteries charged with nitrosulphuric acid or sulphate of copper are examples.

4th. That the law which regulates the connexion of this diminution of quantity or condensation with the increase of tension is the same as that which regulates the analogous phenomena of ponderable elastic fluids.

241. Incidentally, the examination of certain other points will be entered upon: for example, a brief consideration of Lenz's law of the conducting power of wires; this, it will be shown, holds not only in the case of Faradian currents, but in the direct currents from hydro-electric and thermo-electric pairs, as has been advanced by some philosophers, but denied by others.

242. The terms tension, intensity, tensile effect, &c., have had very different significations attached to them. From this circumstance a great deal of confusion has arisen, and it is one of the causes of that diversity of opinion and contrariety of theory which obtain in the elementary parts of the science of electricity. For example, Dr. Faraday appears to use the words *tension* and *intensity* as synonymous, expressive, as it were, of

elastic force, chemical authors generally adopting the same signification: "The remoteness from the unexcited state, a condition expressed by the terms *tension* or *intensity*." "By tension or intensity is meant the energy or effort with which the current is impelled."—(*Turner, Elem. Chem.*)

243. This confusion of terms leads to a confusion of facts of a much more serious kind. English electricians uniformly state that the magnetic needle, deviating in the neighbourhood of a current, takes no note whatever of the intensity of that current. Continental writers, almost without exception, regard the deviation as a function of the intensity, and the statements, therefore, appear discordant. While the effect is thus differently described, all agree as to the facts of the case. In what follows, the term *tension* will be used as expressive of the elastic force of the current, that power by which it is enabled to pass a resisting medium; the term *intensity* will be strictly confined to the acceptation in which writers on analytical mechanics use it. "By the intensity of a force, we understand its greater or lesser capacity to produce motion" (*Boucharlat*); and in the case before us, the intensity will be regarded as a function of the quantity and tension conjointly. Thus, the deviation of a magnetic needle does not indicate the tension, but the intensity of a current.

244. Suppose, now, we had a current of electricity passing under a certain tension, along a channel of conduction, as a bar of large dimensions, and were suddenly to interpose in some part of its path a resisting obstacle, as, for example, a slender wire; it is obvious that a certain portion of the current would pass the barrier, a portion determined partly by the character and dimensions of the wire, and partly by the tension or elastic force of the current. Let the wire under all circumstances be the same, the absolute quantity of electricity be constant, but the tension thereof vary. Now, as the tension increases, the quantity that passes the resisting wire will also increase, and as the one diminishes, so will the other too. Under these circumstances, the absolute quantity that passes will always be an increasing function of the tension; and as this quantity is under all circumstances measureable by the deviations of the magnetic needle, or by the voltameter, these instruments may be used to determine the tension, by making quantity indirectly the measure thereof.

245. If, therefore, we send a certain quantity of electricity, as 100 parts, to a resisting wire, and find that of these 50 parts can pass the obstruction, we may assume such a current to have a higher tension than one containing the same absolute quantity, of which only 30 could pass; and to have a much lower tension than one, of which 70, 80, or 90 parts could pass. In all these cases, the amount per cent. of the main current which passes the resisting wire may be taken as the representative of the tension of that current.

246. This obstructing, resisting wire, I call a secondary wire.

247. But it is plain that this amount per cent., of which I am speaking, in introducing this fundamental proposition, is nothing more than the ratio which exists between the quantities passing the large and the little wires respectively. By dividing, therefore, the quantity that passes the secondary wire by the quantity that passes the large wire, we shall have a numercial representative of the relative tension of the current under consideration.

248. Let us take an example: a single pair of plates developed a current of electricity, which, when measured at the torsion balance, was found equal to 20 degrees; on subjecting this current to a secondary wire, 7 degrees passed it. Its tension might, therefore, be represented by ·3500. A second pair was now added in conformity to the first, 31 parts passing; but when subjected to the secondary wire, 18 were indicated. The tension had now become ·5806: in the same way, by adding three more pairs, the tension rose to ·6346.

249. It must now be borne in mind, that the numerical determinations thus procured are entirely conventional; their absolute value depends upon the resistance of the secondary wire, and they therefore only express the relative condition of different currents.

250. As a considerable advantage will be gained, and much repetition avoided, by here indicating the mode adopted for procuring the following measures, I shall describe at once some modifications and additions which are necessary in the torsion balance, the instrument generally employed.

251. The voltameter has of late come much into use in investigations of this sort, but when compared with the torsion balance, the latter is much more speedy and certain in its indications, and should generally be preferred. In point of fact, the indications of the two instruments are entirely of a different character; the magnetic needle shows the quantity of electricity that is passing in each indivisible portion of time, the voltameter the quantity that has passed at the end of a finite time. In the conditions of the action of the one, time enters as an element, in the other it does not.

252. By applying a glass thread to the needle, the late Dr. Ritchie greatly improved the accuracy and general utility of the galvanometer; but even with that addition, unless certain precautions are taken, the instrument will not work satisfactorily; the motions of the needle are too versatile, and the tremulous state of vibration into which it may be thrown are insuperable barriers to accuracy of measurement. A cylindrical trough filled with water is a perfect and admirable remedy for these difficulties.

253. Another difficulty, which is very generally overlooked, is the excentric position into which the thread is liable to be cast, when the upper micrometer has moved. The construction of the instrument requires that the axis of motion of the upper micrometer, the axis of the glass thread, the axis of the spindle carrying the needles, and the vane, should be in the same vertical straight line, through whatever arc the micrometer may have moved. Now it would be very difficult to accomplish this by any system of adjustments.

254. Whether the instrument is arranged with one or several needles, or whether it has a coil or merely a single strap, the vertical distance from the coil or strap, when the index is brought to zero, ought under no circumstances to vary.

255. In a climate as hot as that in which the following experiments were made, one of the most unpleasant deviations depends on the thread wrenching in the wax, which is used to fasten it to the needles at one end, and to the micrometer at the other; when the wax softens, and the thread is moved through several degrees, it is not the free part alone that undergoes torsion, but also that which is in the wax; hence arises an error as

respects the zero point. This I have always avoided, by ascertaining the zero at the beginning and close of each experiment.

256. After having had some experience with voltameters, deflecting galvanometers, &c., I am induced to describe the instrument used in these experiments, for it will enable those who are not accustomed to the torsion balance to execute measures very easily, which they might otherwise ineffectually attempt.

257. A A, B B (*fig.* 28), is a glass jar, 16 inches high, open at both ends; at A A it is 2½ inches in diameter, at B B 6 inches; it rests upon a piece of wood 8 inches by 10. A strap of stout sheet-copper, *e f f e*, 1 inch wide and 15 long, is bent into the form indicated; its extremities at *e e* being let into the wood, and bearing mercury boxes, D D. The central part of this strap, from *f* to *f*, is placed horizontally, and has a circular aperture and side gap, as is shown in *fig.* 29, *a a*, through which the spindle carrying the needle can be passed, and works.

258. The upper extremity of the jar, A A, is accommodated with a divided circle, in the centre of which the key G works: this key is ground, like a stopcock, to a slightly conical figure; it therefore revolves very truly without any shake: it is drilled longitudinally to admit the passage of the glass thread, which is secured in it by means of a perforated straw and a drop of sealing-wax.

259. The other extremity of the thread enters a little tubular perforation in the ivory axis, *n n′*, and is also secured therein by wax. Only one needle is used; it is lozenge-shaped, and is 4½ inches long. Besides carrying this needle, the ivory axis extends an inch and a half below it, and in a slit at its lower extremity, confines a parallelogram of stout tinfoil, *r r*, an inch wide, and 2¾ long. When in use, this vane of tinfoil works in a glass cup, *k k*, 3½ inches in diameter, which is filled with water.

260. One of the chief improvements in the instrument is connected with the needle, and the axis on which it works. The latter is a small cylinder of ivory; it has two flat faces filed upon it, corresponding to the direction of the needle. On each of these faces, as is represented in (*fig.* 30), is drawn a vertical line, and a little to the right of it are placed five dots. The polar extremities of the needle are accommodated with two upright wires, *p p′*, *p p′*, an inch long, which serve as indexes; and at a distance of 10 or 15 inches, in the magnetic meridian, a plate of metal, not shown in the figure, with a small hole in its centre, is placed, to be used as a sight. When an observation is to be made, the experimenter adjusts this sight in front of the instrument, either on its north or south side; and on looking through it, as soon as the needle moves, he sees the index, *p p′*, traverse *before the scale on the axis.* There is no shake or vibration, even though any one should cross the floor or jar the table, for the index and the scale equally participating in all these disturbances, the motion is almost as steady as that of a shadow on a sundial; the vane of tinfoil does not in the least interfere with the accuracy of indication, but effectually stops the oscillations, and the utmost accuracy may be obtained, by previously giving the index, *p p′*, a slight bend out of the vertical line, and using the five dots as a diagonal vernier.

261. In the following memoir, it will be seen that the terms primary and secondary wire are occasionally used, the former in a somewhat extended sense: I mean by it

not only the thick polar wires that come from the electromoter, those which were used being one fifth of an inch thick, but include the electromoter itself, no matter what its character may be—if a hydro-arrangement, the plates, exciting liquid, &c. The secondary wires are simply long or slender wires, to obstruct the current; of these I have occasionally used two, the first 47 inches long, the second 290: they are of copper, one foot of which weighs 10·65 grs., and are covered with silk.

262. And, lastly, the measures are sometimes arranged in a form such as this:

$$\left.\begin{matrix}100\\50\end{matrix}\right\}\cdot5000,$$

in which the large or upper number represents the quantity passing the primary wire, the under or smaller number the quantity passing the secondary wire, and the decimal on the right hand of the bracket, being the quotient of the former numbers, is, as will presently be shown, the representative of the tension.

263. We have now to examine the foregoing proposition more minutely. Let us call the primary wire, being that which is in connexion with the electromotoric source, A; and the secondary or resisting wire, B. Now how does B act towards currents when they are of variable character? There is no current, no matter how low its tension may be, that will not pass along B to a certain extent: this is abundantly proved by such a wire transmitting a thermal current of the lowest tension and amount. But at the other extremity of the scale is there a limiting point? Can a wire conduct electricity of a certain tension only to a certain amount? I think not, for a wire of small diameter was found upon trial to conduct a thermal current to the extent at one time of 20, and then of 284 parts, the tension in both cases being the same; and if it would do this in the case of currents whose tension is so very low, the same might be looked for in hydro-currents; here, however, when the quantity reaches a certain point, the ignition of the wire ensues, and its physical character is changed. Sir Humphrey Davy's experiments lead to the same conclusion (*Phil. Mag., Dec.*, 1821), nor does there appear to be any limit to the conducting power of a wire, either for high or for low tension. If a wire carries a certain amount of electricity, an increase of quantity or of tension will enable it to carry more, and the converse. To this important point I shall presently return.

264. As it thus appears that any increase of the quantity which A transmits involves also an increase of that which passes B, a second question arises, What is the ratio that will be observed in the two cases? If the quantity passing A be doubled, will the quantity passing B be doubled also? This is a very important problem; for if the ratio above mentioned holds, it would show that an observation by the secondary wire will give the tension independent of the absolute quantity. Let a represent the quantity traversing A, and b the quantity traversing B. Now, if the tension remains constant, and the quantity only is variable, the ratio $\frac{b}{a}$ is always constant, and is entirely independent of the value of a.

265. This I have endeavoured to prove experimentally. I took a hydro-electric pair of copper and zinc, each of the plates exposing about two square feet of surface, and dipped them to different depths in dilute sulphuric acid. The following table exhibits one of these results:

TABLE A.

Primary Wire.	Secon. Wire.	Calculated.
49	34	34
37	26	25·6
24	17	16·6
13	9·50	9·0

and therefore we infer that the foregoing ratio holds.

266. Currents of very low tension give proofs of the same fact. A thermal pair of platina and palladium passed 44 through the primary, and 19·50 through the secondary wire; and when, by increasing the temperature, 236 passed through the primary, 115 went through the secondary wire. In a pair of palladium and silver, 165 and 1130 being passed successively through the primary, 43 and 313 went through the secondary wire. In a pair of iron and platina, 170 and 249 being successively sent through the primary, 79 and 112 respectively passed through the secondary wire.

267. But let us farther suppose that the quantity of electricity passing at different times through the primary wire A is constant, its tension alone undergoing an increase. If A formerly conducted all that was presented to it, it will, under this new condition of things, of course still do the same. Such, however, will not be the case with B, for a greater quantity is now enabled to pass it than before, and the ratio $\frac{b}{a}$ will give a greater value; we shall therefore, in this case, have a measure of the tension. But if the tension still keeps increasing, b will continually approach to equality with a; and when the tension is infinitely high, these quantities are accurately equal to each other; or, in other words, when the elastic force of a current is infinitely high, its tension is unity.

268. If, on the other hand, the tension becomes lower and lower, b continually decreases, and, finally, might be found equal to zero. The value of the ratio then becomes zero; and, therefore, at the two extremes, or where the tension is unity and where it is zero, the secondary wire, so far from ceasing to act, still truly indicates the condition of the current.

269. While, therefore, A conducts freely the whole current, B will measure its tension under all circumstances; but, in point of practice, we can never make the adjustment here hypothetically indicated, or so arrange a wire A that it shall conduct *all* the electricity presented to it. Let us, therefore, here inquire how this variable condition of *both* wires will affect the result. Let the tension (t) so change by any amount as to become ($n\ t$), then a corresponding change will happen in a and b, admitting the principle that the quantities passing through A and B are increasing functions of (t). If, then, (t) becomes ($n\ t$), a will become ($n\ a$). Now, if the equation $\frac{b}{a} = t$ holds, $b = a\ t$; but, when the change impressed on (t) has happened, b will be equal to the conjoint values of ($n\ a$) and ($n\ t$); and, if these values be substituted in the former ratio, the result is still equal to ($n\ t$); so that, whatever may be the change impressed on (t), the formula $\frac{b}{a} = t$ will always indicate it.

270. Having thus settled, by the foregoing simple reasoning, the fundamental doc-

trine of investigation, I next proceed to apply it to the analysis of the different processes, by which a change of tension is supposed to be impressed on an electric current; and this leads to the consideration of the second proposition:

271. "That there is reason to doubt whether the processes usually supposed to affect the condition of an electric current are ever attended with any such result; but that, when changes have apparently taken place, it is probable that they may be directly traced either to a disturbance at the place of generation, or to the development of other currents of a different character, the primary current itself remaining unchanged."

272. It is popularly supposed that if we pass an electric current through a wire of certain length, coiled upon itself, a kind of inductive influence will be exerted, so that the current shall become more and more intense as it goes. Or, if two currents are simultaneously passed into a double helix, they will mutually fortify each other.

273. (*a.*) A wire, covered with silk, 48 feet long, and arranged as one circular arc, had a current passed through it which produced a deviation of 35 degrees. The same wire was then coiled round a piece of wood, so as to make 155 circumvolutions; the deviation was still 35; and, therefore, no change was impressed on the current.

274. (*b.*) A thermal current was passed through a straight wire with the following result:

$$\left.\begin{matrix}42\\22\end{matrix}\right\}\cdot 5238.$$

The wire was then coiled into a helix, the current passed through it and measured; a powerful bar magnet was next introduced into the helix, and then a rod of soft iron. But in all these cases the measured numbers were absolutely the same as before. Therefore there is no change impressed on the thermal current, either in relation to quantity or tension, by making it pass along a coiled wire, or by acting on it with a magnet or a bar of soft iron.

275. (*c.*) The same experiments were made with a hydro-electric current, and they gave the same results.

276. (*d.*) The above-mentioned (*b*) thermal current was passed along one of the wires of a double helix, and through the other wire a hydro-current was passed, from a single pair of plates; but the tension and quantity remained the same as before. On sending a current of still greater intensity, viz., from a voltaic series of five pairs of plates, the same result was still obtained; the hydro-current had power enough to decompose water.

277. (*e.*) On altering the polar communications, and thereby changing the course of the current, no change whatever in the primary current, either as to quantity or tension, was observed.

278. It is well known, that by using a long wire as a discharger of a single pair of plates, a spark will be obtained of a much more brilliant character than when the current passes through a shorter wire; it is upon this fact that the flat spiral riband coil is constructed. Many electricians have supposed that the results obtained by this beautiful contrivance were partly due to the inducing action of the successive spires, but chiefly to a long and easy conducting channel being open to the current, which gathers momentum in its passage. I have already shown that there is no *permanent* action

of induction in the case of a coiled wire, an observation applying equally to an elongated helix and to a flat spiral. Let us now determine whether the increased tension is due to momentum.

279. A copper wire 46 feet long and $\frac{1}{16}$ inch in diameter being arranged as the discharger of a single pair of plates, a brilliant spark was seen to pass; but with a wire of the same diameter and a foot long, the spark was barely perceptible. The quantity and tension in each case were now determined.

TABLE B.

Short wire . . .	39 / 12	·3076
Long wire . . .	13 / 8	·6153

Hence, by the use of a long wire, we greatly increase the tension of an electric current. A second experiment, in which a wire $\frac{1}{8}$, and a third, in which a wire $\frac{1}{5}$ of an inch in diameter, were used, gave analogous results. In neither of these cases, however, did the tension rise so high as in the former; it was lower as the diameter of the wire was greater.

280. This increase of tension follows the increase of the length of the wire, as the following measures show.

TABLE C.

Ex.		Quantity.	Tension.
1.	Current from a single pair of plates	79	·4177
2.	——— a long wire introduced	44	·5909
3.	——— a second ditto., added	27	·7222
4.	——— third	21	·7619
5.	——— fourth	15	·8333

Thus, by successively increasing the aggregate length of the discharging wire, the tension continually increased, commencing at ·4177, and finally becoming ·8333. Similar experiments with other wires gave similar results.

281. Now is this remarkable rise of tension due to a momentum which the current acquires on the wire? Or does it arise from the fact, that the wire acts simply as an obstacle, reacting thereby on the electromotoric plates, the increase of tension being due to them, not it? This is easily determined; for if the rise of tension be due to the plates and not to the wire, a short wire, slender enough to obstruct the current to the same extent, ought to act equally as well as the long wire.

282. This experiment, the result of which leads to the true theory of voltaic combinations, I shall carefully describe.

283. I took a copper wire 46 feet long and $\frac{1}{16}$ inch in diameter, and found that it stopped a certain portion of the current coming from a single pair of plates. The micrometer of the balance was now turned, and the needle brought accurately to zero. Then I cut off from another slender copper wire such a length (2 feet 10 inches) as to obstruct the current to the same extent as the long wire, the needle being brought, when it was interposed in the path of the current, to zero. The secondary coil was now introduced; it of course stopped off a certain portion of the current; but the micrometer was again adjusted, until the needle was brought to zero. And now the long

wire being introduced, and the slender one taken away, the needle came again to zero. But I suppose, if the long wire had impressed more tension on the current than the slender one, either by momentum or otherwise, more electricity should have passed the secondary wire when it was used, which is not the case.

284. Again, I took a copper wire 242 feet long and $\frac{1}{16}$ inch in diameter, and adjusted to it a fine iron wire as before: the extremities of this wire were tinned; it was $12\frac{1}{2}$ inches long. Either of these wires being used as a discharger, brought the needle to the same point of the scale. On using the secondary wire and the long wire together, I adjusted the needle accurately to zero, and then passing the current through the fine wire and secondary wire, it came again to zero. And this was repeated often, and so near was the adjustment, that, when an assistant turned first one and then the other wire on, it could not be told which was in action, or whether the current had come along the long or the short wire. A long wire, therefore, impresses no sort of change on a current, but merely serves as an obstacle; for, in the first case, we had one wire sixteen times longer than the other, and in this we have a wire more than 230 longer than the one with which it is compared, yet the tension has increased only to the same amount in both.

285. And the same results were obtained by the voltameter.

286. The current that flows in a simple closed voltaic circle may be resisted in two ways: 1st, the length of the wire connecting the plates may be increased, as in the foregoing experiments; 2d, the connecting wire remaining of constant length, the *distance of the plates* may be increased: the result is the same in both cases, a rise of tension.

TABLE D.

Ex.	Distance of the plates in inches.	Quantity.	Tension.
1	. . . . . . 2·75	111	·7297
2	. . . . . . 4·50	50	·7600
3	. . . . . . 9·00	27	8888

So that, whether we obstruct the current by lengthening the connecting wire or by increasing the distance of the plates, the general effect is the same, the tension immediately rises; that increase of tension being due to the plates themselves, and not to the channel of conduction. This brings us to the third proposition,

287. "That there are two different methods of accomplishing these disturbances, and thereby of raising the elastic force of a current. 1st. That tension may be augmented by the sacrifice of quantity; VOLTA's plan of a reduplicated series, and HENRY's riband coil in its condition of equilibrium, being examples. 2d. By the introduction of new affinities in the exciting cells; batteries charged with nitrosulphuric acid or sulphate of copper are examples."

288. A single pair of plates, under the influence of a long wire, or the spiral coil, presents a remarkable analogy to Volta's pairs arranged in reduplicated series. In point of fact, they may be considered as scarcely differing from each other either in mode of action or in effect. The study of the single pair under this condition reveals at once the theory of the voltaic action.

289. If we inspect tables B, C, D, we are at once furnished with the fundamental fact which is the basis of explanation. When we compare together the tension and

quantity of the electricity flowing in the primary wire, we are struck with the fact, that whenever the one has increased, the other has diminished. *No matter what the other conditions may be, whether the communication is made by a long wire or a short one, whether the plates are near or far apart, whenever the quantity is diminished, the tension increases; and whenever the quantity increases, the tension is diminished.*

290. The remarkable analogy of the ponderable elastic fluids, which, when their volume is diminished, or, in other words, condensation takes place, experience an increase of tension or elastic force, is here too broadly indicated to be mistaken.

291. When I first saw that removing the plates to a greater distance apart determined a given rise in the elastic force of the current, for a time it appeared to me that Dr. FARADAY's theory of the tension being due to the affinity of the zinc for oxygen must certainly be incorrect. A more extensive acquaintance with the facts has reversed that opinion. If the tension be determined by the affinity of the metal for oxygen, which must be a constant force, how comes it to pass that moving the plates to a greater distance apart can cause it to increase? This apparent paradox, when properly understood, forms a fine illustration of the truth of the doctrine advanced in the 5th, 7th, and 8th series of that philosopher's researches. In what follows I shall, therefore, regard those doctrines as established.

292. Let us take a given pair of plates, and connect them together by a slender wire. We find that the quantity that the plates generate is diminished, and its tension is increased; but that this has not happened either by gain of momentum or inductive influence in the channel of communication, and we are compelled to refer the effect to the resistance of the wire, placing the plates and the electrolyte between them in a state of force. If this be the action of a resisting medium, we might suppose that by continually increasing it we should continually increase the tension, and when it became infinitely great, the tension would be so too. But what is the true action of a slender wire, connecting in this way a pair of plates? A certain amount of electricity passes along it, but not the *whole quantity* that the plates could generate in *a given time;* yet we cannot suppose that *all* that does pass comes from the *whole surface exposed,* and not from a *fractional part* thereof. The water and zinc are ready to generate, and, as it were, attempting to drive a fresh quantity of electricity through the wire; and, accordingly, as the quantity that actually passes becomes a greater and greater portion of what the system actually tends to put in motion, the tension becomes less and less. The tension would therefore become zero if the whole circle wires, plates, and electrolyte could carry all that the zinc and water could generate. The limit prescribed to its diminution is the conducting power of the electrolyte, which is the worst conductor of the system.

293. This hypothetical condition, of a tension ranging near zero, is most nearly approximated to in a thermal pair.

294. Suppose, now, that everything remains the same as respects wires, electrolyte, distance of plates, &c., except that the dimensions of both plates are doubled. Shall we increase the tension? No; for although the surface in action is doubled, and the absolute quantity which the system could generate is doubled, yet the quantity that

passes both the primary and secondary wire is also doubled: the ratio $\frac{b}{a}$ is therefore the same as before. For this reason, increasing the magnitude of the plates increases the quantity only, and not the tension.

295. Under all these circumstances, therefore, the tension depends on the ratio of the quantity that does pass the combination, to the quantity that the system tends to put in motion.

296. Before, however, we can go farther in the study of these conditions of tension, or attempt to show that the arrangement of Volta, and a single pair under the influence of a long or thin wire, are, in point of fact, alike in principle, it is necessary that we should understand the nature of the different disturbing actions that may arise in the generating cells of the electromotor.

296. I took a zinc plate 7 inches long and 3 wide, and a corresponding copper: the surface of the former was amalgamated and the latter brightened. The plates were fixed at an immovable distance from each other, and immersed in a jar containing 34 ounces of water. SULPHURIC ACID was then added by half drachms successively.

TABLE E.

Exp.	Quantity of acid in drachms.	Quantity.	Tension.
1.	½	20	·555
2.	1	39	·436
3.	1½	55	·382
4.	2	72	·305
5.	2½	83	·277
6.	3	98	·245
7.	3½	112	·223
8.	4	121	·214
9.	8	216	·130

298. Here we have an exemplification of the converse of the fact already so much insisted on. As the quantity developed from the *same surface* gradually became greater and greater, the tension became less and less, due to the increased conducting powers of the fluid medium. It is the same effect that would have been produced by constantly shortening the connecting wire.

299. Such is the action of increasing doses of sulphuric acid; let us now see how NITRIC ACID will act. The copper plate being repolished, and the zinc reamalgamated, and everything else being as at the commencement of the former trial, the latter acid was now added to the cell in the same way that the former had been used.

TABLE F.

Exp.	Quantity of acid in drachms.	Quantity.	Tension.
1.	½	14	·7143
2.	1	22	6363
3.	1½	36	·5555
4.	4	175	·2400

These measures are effected with some difficulty, as the acid acts somewhat irregularly, and keeps the needle vibrating.

300. Muriatic acid, under the same conditions and circumstances, being substituted, gave as follows:

TABLE G.

Exp.	Quantity of acid in drachms.	Quantity.	Tension.
1.	½	10	·8000
2.	1	17	·6530
3.	1½	23	·6087
4.	2	29	·5517
5.	2½	34	·5000
6.	3	39	·4872
7.	3½	44	·4654
8.	4	49	·4387
9.	8	82	·3169
10.	16	145	·1938
11.	24	203	·1707

301. Nitrosulphuric acid, the constituents of which were added alternately in equal measures, was next tried.

TABLE H.

Exp.	Acid.	Quantity of drachms.	Quantity.	Tension.
1.	Sulphuric.	½	23	·5652
2.	Nitric.	½	53	·5094
3.	Sulp.	1	72	·3888
4.	Nitr.	1	94	·3510
5.	Sulp.	2	178	·2360
6.	Nitr.	2	190	·2026

302. Solution of sulphate of copper was next experimented with.

TABLE I.

Exp.	Quantity.	Tension.
1.	33	·8181
2.	74	·6081
3.	116	·4741

These measures were procured with difficulty, owing to the flocculent deposit which settled on the zinc, more rapidly as the solution was made stronger.

303. In the general discussion of the measures given by tables E, F, G, H, I, we still see the operation of the same general law, that the tension rapidly diminishes as the acid is added, and that when the same quantity of electricity developed from the same amount of surface by these different acids is presented to the secondary wire, the quantities that can pass that wire are very different; and on making use of these different agents, it would appear that they can give rise to currents from the same metalline surface, equal in point of quantity, but differing greatly in point of tension, in the following order, beginning with the most powerful:

Sulphate of copper,
Nitric acid,
Nitrosulphuric acid,
Muriatic acid,
Sulphuric acid,

304. Of these bodies, the muriatic acid acts probably in the way that Dr. Faraday has pointed out, but the immediate cause of the rise of tension in the others is to be traced to the circumstance that they furnish oxygen to the nascent hydrogen; and if the tension of the ordinary current is made to depend on the tendency of the zinc and oxygen to unite, it is reasonable to suppose that that tension will increase if a new affinity be introduced, the action of which should correspond with and abet that of the zinc for oxygen. This takes place when nitric acid or an oxysalt of easy decomposibility is added to the solution. The tension of the current is not then determined by the af

finity of the zinc for oxygen only, but by *all* the affinities that can take place among *all* the bodies in the exciting cell. We are, therefore, here led to expand Dr. Faraday's theory, and to regard what follows as directly opposed to the theory of contact.

305. Upon these principles, in an ordinary arrangement of copper, zinc, and sulphuric acid, the tension of the current is determined, by the sum of the affinities of zinc for oxygen, and hydrogen for copper, diminished by the sum of the affinities of copper for oxygen, oxygen for hydrogen, and hydrogen for zinc.

306. But as, under all ordinary circumstances, the affinities of hydrogen for zinc and copper may be neglected, they being exceedingly small in comparison with the others, we may assume,

307. That the tension of the current is equal to the affinity of oxygen for zinc, diminished by the sum of the affinities of hydrogen and copper respectively for oxygen.

308. If now we introduce into the exciting cells nitric acid or sulphate of copper, the affinity of the nascent hydrogen for oxygen is satisfied, and the resistance from this source is nearly exterminated, and the tension of the current is then equal to the difference of the affinities of zinc and copper for oxygen.

309. By thus exterminating the resistance arising from the affinity of hydrogen for oxygen, we succeed in raising the tension greatly; if next we get rid of the affinity of copper for oxygen, the tension ought to become still higher. This may, in a measure, be effected by making use of a plate of platina, as I found experimentally.

310. In all these cases, in which the tension increases without loss of quantity, we directly trace the action to a disturbance in the exciting cells. In ordinary voltaic arrangements, the maximum tension is never reached, because the affinity of zinc for oxygen, which determines the current, is counteracted to a certain extent by the affinity of oxygen for hydrogen. If we satisfy that affinity, an increase of tension is the result, and accordingly as this is more and more nearly effected, more and more of the hydrogen that ought to be evolved disappears. This remarkable disappearance of hydrogen has been heretofore noticed, but the true office it served has not been detected. If a battery is charged with nitrosulphuric acid, the hydrogen evolved is no longer the equivalent of the zinc expended; in point of fact, the gas evolved is no longer hydrogen, but a mixture of hydrogen and the binoxide of nitrogen, as is proved by its burning with a green flame. I took a small pair of plates, the zinc being amalgamated and the platina freshly cleaned, and placed them in a mixture of six ounces of water and one drachm of sulphuric acid, arranging an inverted tube over them, so as to collect the gas from the platina plate. I determined by weighing the zinc plate how much was expended in evolving a given quantity of gas; and then successively adding sulphuric acid until the total amount had reached eight drachms, it appeared that in each instance it required very nearly 1·79 grains of metal; but on adding one drachm of nitric acid to the mixture, the quantity expended rose at once to 2·25 grains, and on adding a second, to 3·00 grains.

311. Therefore, unless care is taken that no oxidizing body is present, the voltameter will give deceptive results. This important precept should be perpetually borne in mind by those who employ it in investigations. A few drops of nitric acid will at

once vitiate its indications; and there is reason to suspect that, under certain circumstances, even the dilute sulphuric acid with which it is charged may undergo partial deoxidation, and the evolved hydrogen indicate an amount of electricity less than is actually passing.

312. We are, therefore, in possession of two distinct methods of indirectly increasing the tension of an electric current. The first depends on the reduction of quantity; the second, on satisfying in the exciting cells affinities which tend to antagonize that which determines the current.

313. Volta's plan of a reduplicated series unquestionably acts upon the first of these principles. It is a fact admitted on all hands, and, therefore, into the proof it is unnecessary now to go, that the apparent quantity circulating in the whole battery is not greater than that which any one of the pairs could generate. Dr. Faraday has already shown how an enormous quantity of zinc is thus expended, the equivalent of electricity being entirely sacrificed for the sake of increasing the tension. Let us see what are the facts in the case. The first pair of plates develops by the oxidation of a portion of its zinc a certain quantity of electricity, which, passing through the electrolytic conductor, arrives at the second cell; here, however, it is stopped, as a transit without decomposition is impossible, a decomposition which it is unable to effect. Continually tending to pass, without the passage actually taking place, it remains, as it were, on the surface of the second zinc plate, in a condensed state, reacting on the electricity which that plate is generating, compressing and being compressed by it, and, therefore, increasing its elastic force. And the same action continually occurs, and increases the tension throughout the series.

314. A flat spiral coil, or a long connecting wire, obviously acts in the very same way. It opposes a resistance to the passage of the current, and the plate instantly becomes in a forced state. We might almost regard the electric fluid as existing upon the surface of the zinc, exerting to the utmost its elastic force to pass the barrier, and failing that, compressing the evolved fluid as fast as it is generated, and being compressed by it. This, also, is the case in the pile of Volta.

315. Thus far, therefore, the riband coil acts simply as a long wire, and this may be regarded as its primary or statical effect. But, besides this, it gives rise to an action of an entirely different character, which Professor Henry pointed out and explained. In the act of making and breaking contact in a system of which it forms a part, Faradian currents are generated by its successive spirals; these currents under the latter condition, breaking contact, coincide in direction with the primary current then just ceasing to pass. We must, however, carefully distinguish between these currents and that which induced them. In this respect some philosophers have unguardedly fallen into a very remarkable mistake; it has been supposed that when a thermo-electric current was passed through this coil, and a spark obtained, the thermal light was seen! The case is exactly analogous to that in which similar coils pass the jaws of a horseshoe magnet; no one supposes that the spark then elicited is due to the electricity of the magnet itself, but is simply a manifestation of the induced current; the very same thing takes place when the thermal current runs through the spires of a flat coil. So

far as I am informed, the magnetic spark and the true thermo-electric spark have never yet been seen.

316. These observations are made in order that I may not be misunderstood. It is not my object to consider the different arrangements that can generate a Faradian current, and, therefore, in this point of view I dismiss the flat spiral.

317. We now come to the fourth and last proposition, which is, "That the law which regulates the connexion of the diminution of quantity or condensation with the increase of tension, is the same as that which regulates the analogous phenomena of ponderable elastic fluids."

318. I have not hesitated to use the terms "compression," "condensation," "elastic force," in reference to electricity, though I well know such an application is unusual. But it has seemed to me that a single pair might *almost* be likened to a steam-engine boiler, from which if you let the steam escape by a wide tube, its elastic force is less and less, accordingly as the escape is more free; but, if you put upon it a narrow tube, the vapour rushes with vehemence through it, reaction in a moment occurs in the boiler, the elastic force increases, and the accumulated steam pressing heavily on its surface, the water boils in a more laboured way: this narrow tube resembles HENRY's coil, or a long or slender wire.

319. The following table exhibits numerical results obtained by the aid of one of DANIELL's constant batteries, the tension being continually increased by the addition of successively increasing lengths of wire

TABLE K.

		Quantity.	Calculated.	Tension.
Without any wire interposed.	Beginning of the experiment,	76		·8289
	End of ditto,	75		
Wire interposed	6 feet long	72	72·00	·8333
" "	12 "	68.50	68·30	·8394
" "	18 "	65	65·24	·8461
" "	24 "	62	62·30	·8548
" "	48 "	53	53·12	·8679
" "	72 "	46	46·18	·8913
" "	96 "	41	41·00	·9219

320. From this table it would appear that the addition of successively increasing lengths of wire of invariable diameter diminishes the absolute quantity of electricity flowing, but at the same time the tension is exalted. By taking the angle of torsion as the measure of the forces, in the second column, it is also evident that the law of the conducting power of wires given by M. LENZ holds in the case of a hydro-electric pair. This may be regarded as of some interest, inasmuch as the late Dr. RITCHIE, in certain papers read before the Royal Society, opposed to the very last this view, by the aid of numerical determinations made with the torsion balance, the instrument here employed. In reference to the third column of the table, I have calculated it in the manner given by LENZ, the value of the constant to be deduced from the reciprocals of the angles of torsion being in this case 1318 nearly.

321. While, therefore, these results confirm, in the most pointed manner, the reasoning of that able philosopher, they at the same time compel us to advance a step far-

ther, and to expand, to a certain extent, OHM's theory of the voltaic pile. It is a condition, in tracing the action of wires of different lengths, to assume that the electromotive power of the generating pair is under all circumstances constant, and hence it may be conveniently represented by unity. But the electromotive power of any pair plainly depends on *two* things: the quantity of electricity that the pair can evolve, and its absolute tension. The theory of OHM, as may be gathered from the memoir of Professor JACOBI on electromotive machines, and also from M. LENZ's papers, confounds those two important conditions.

322. Now the results given in the foregoing table, proving that wires conduct in the inverse ratio of their lengths, prove, also, that the addition of increasing lengths of wire does not in any wise alter the electromotive power; yet we have clearly shown that this addition is inevitably attended with an increase of tension. Here, therefore, is an apparent contradiction.

323. But this contradiction is only apparent, and, when properly understood, leads to a most remarkable result.

324. It is true that we are compelled to assume that the electro-motive power of a pair is independent of the length of the connecting wire; but this constancy of electromotive power does not necessarily imply that the relations of quantity and tension, which conjointly produce it, are not themselves variable. In the case before us, we have direct proof that the tension increases, and also that the quantity decreases, as the connecting wire becomes longer, and the converse; yet the electromotive power varying directly with them both, they must of necessity bear such a relation to each other, that their product shall always be equal to unity. Hence we infer,

325. That the law of MARRIOTTE in relation to the ponderable elastic fluids holds, also, in the case of electricity developed by voltaic action, the elastic force or tension of a given quantity being inversely as the space it occupies.

326. The following table will at the same time establish LENZ's law in the case of thermo-electric currents, and prove that even in cases where the tension is so exceedingly low, the elastic force of a given quantity of electricity follows the above-named law.

TABLE L.

	Quantity.	Calculated.	Tension.
Without any wire . . .	720	. . .	·0930
Interposed wire 1 foot long	298	298	·2080
" " 2 feet long	192	192	·3021
" " 3 "	142	142	·3802
" " 4 "	112	113	·4375
" " 5 "	93	93	·4731
" " 6 "	80	80	·5000

The current here experimented with was generated by a pair of wires of copper and tinned iron $\frac{1}{12}$ inch in diameter, and one foot long, the soldered extremity being immersed in a bath of boiling water, and the free extremity carefully maintained at $59\frac{1}{2}$ Fahr.; the third column in the table being calculated by the aid of the constant 1527.

327. As respects electricity of high tension, a law extremely analogous to that here indicated may be traced. The striking distance varies directly as the quantity accumulates. If a given jar be successively charged with quantities of electricity, as the numbers 1, 2, 3, 4, &c., the intervals of air through which the spark can pass vary di-

rectly as those numbers. This is abundantly shown by the experiments of LANE, HARRIS, and other philosophers.

328. Now upon what does this striking distance depend? Plainly upon the elastic force of the coerced fluid, and therefore the striking distance will measure the elastic force or tension. We condense upon a given surface increasing quantities of the electric fluid, and find that the law in relation to its elastic force is, that the tension of a given quantity is inversely as its volume. But this is the law of MARRIOTTE in relation to the ponderable elastic fluids.

329. The following numerical determinations were made by adding successive plates to the first single hydro pair, and taking the values of the current on each addition. It is offered merely as an illustration of the chief fact under discussion, and is not to be regarded as absolutely correct, though every precaution was taken to avoid changes in the current. It shows the decrease of quantity and the increase of tension in VOLTA's instrument. Of course, in reasoning upon it, the hypothetical action of each plate is assumed to be equal to that of any other in the series.

TABLE M.

No. of Plates.	Quantity.	Tension.
1.	20	·3500
2.	31	·5806
3.	43	·5814
4.	51	·6274
5.	52	·6346

330. Thermo-electric piles are well known to give the same general results, as respects tension, that hydro-electric piles do; they are much better suited to the purpose of the experimenter, and give currents that are far more constant. The following table represents the action of such a battery, consisting of wires of copper and tinned iron, each element being about one foot long and $\frac{1}{12}$ inch in diameter. The source of heat was a bath of boiling water.

TABLE N.

No. of Pairs.	Quantity	Tension.
1.	256	·1367
2.	305	·2065
3.	325	·2707
4.	348	·3072
5.	352	·3204
11.	396	·5275

331. The beautiful experiments of BECQUEREL, and the equally elegant repetition of them by Dr. GOLDING BIRD, show that the view I have here taken of the action of a single pair is correct. The latter chemist found, that not only could a single pair decompose bodies, such as iodide of potassium, &c., which easily yield up their elements, but that the ammoniacal amalgam might be formed, potassium reduced, and, in point of fact, any decomposition effected. And what is the plan followed? The current is forced to pass, in the electrolyte that is to be decomposed, an obstacle or resisting medium; the tension instantly rises, but at a vast sacrifice of quantity, so that the magnetic needle, which measures only the quantity passing in an indivisible portion of time, is barely affected. Yet, by continuing the current for a great length of time, the resulting decomposing effects are finally the same as those obtained more speedily by the action of many pairs.

332. How far the experiments given in this memoir bear upon that part of Dr. Faraday's researches, in which he has determined the relation of common and voltaic electricity by measure, would form a most important subject of investigation. The results at which he arrives are in themselves very astonishing, and are fully borne out by his decisive experiments; but when we come to reflect that these results were obtained by the magnetic needle and electro-chemical action, we may, perhaps, pause. We may ask whether it is possible to determine by either of these means the *absolute* quantity of electricity that passes. Both measure, so to speak, the volume that flows, the one in an indivisible portion of time, the other that which has flowed at the end of a finite time; but do either of them measure the *true absolute* quantity? Can we tell the absolute amount of a gas without first knowing its condition as to condensation? Can we know *how much* electricity is upon a prime conductor, or compare it with that evolved by a voltaic pile, without first knowing *its* state of condensation? I shall be excused for employing these expressions in an unusual way, and for reasoning about this subtle agent as though it were a ponderable body, inasmuch as it serves, without introducing any hypothesis, to give us more tangible and distinct ideas of what we might otherwise vainly attempt to express.

333. In the December number of this journal (*L. and E. Phil. Mag.*, vol xiii., p. 401), which has just reached me, I find some remarks of Dr. Jacobi on the galvanic spark. Some time ago I came, by another method of experimenting, to the same conclusion. If this spark be really projected by the tension before contact, it ought to take effect at an unlimited distance in a perfect vacuum; but it will be found, on making the trial, that if an iron electrode be sealed into the upper part of a barometer tube, and the mercury made to rise gradually towards its point, the spark does not pass until apparent contact takes place: this was found in an analogous, but vain attempt to show the thermo-electric spark. It cannot, however, be entirely, as that philosopher supposes, "simply a phenomenon of combustion," as it is difficult to understand how mercury can enter into combustion in a vacuum.

CHAPTER IX

ON THE ELECTROMOTIVE POWER OF HEAT.

(*From the London and Edinburgh Philosophical Magazine for June*, 1840.)

Contents: *Object of the Memoir.—Experimental Arrangement to determine the Electromotive Power.—Temperatures calculated from Quantities of Electricity.—Increase of Tension with increase of Temperature.—Depends on increased Resistance to Conduction.—Quantity of Electricity independent of heated Surface.—In Thermo-electric Piles, the Quantity of Electricity proportional to the Number of Pairs.—Best Forms of Construction of Thermo-electric Pairs.*

334. From the memoir of M. Melloni, on the Polarization of Heat, inserted in the second part of the first volume of the Scientific Memoirs, we learn that M. Becquerel,

as well as himself, has made experiments to determine the quantities of electricity set in motion by known increments of heat. From these experiments, they conclude that through the whole range of the thermometric scale, those quantities are directly proportional to each other.

335. But as thermo-electric currents are now employed in a variety of delicate physical investigations, and as there appears to be much misconception as to their character, I propose in this memoir to show,

1st. That equal increments of heat do not set in motion equal quantities of electricity.

2dly. That the tension undergoes a slight increase with increase of temperature; a phenomenon due to the increased resistance to conduction of metals, when their temperature rises.

3dly. That the quantity of electricity evolved at any given temperature is independent of the amount of heated surface; a mere point being just as efficacious as an indefinitely extended surface.

4thly. That the quantities of electricity evolved in a pile of pairs are directly proportional to the number of the elements.

336. First, then, as to the comparative march of electric development, with the rise of temperature, in the case of pairs of different metals.

337. The experimental arrangement which I have employed is represented in *fig.* 31. A A is a glass vessel about three inches in diameter, with a wide neck, through which can be inserted a mercurial thermometer, *b*, and one extremity of a pair of electro-motoric wires. The wires I have employed have generally been a foot long, and one sixteenth of an inch in diameter. The extremity, *s*, of the wires thus introduced into the vessel ought to be soldered with hard solder; their free extremities dip into the glass cups, *d d*, filled with mercury, and immersed in a trough, *e*, containing water and pounded ice. By means of the copper wires, *f f*, one sixth of an inch thick, communication is established with the mercury cups of the galvanometer. The coil of this galvanometer is of copper wire one eighth of an inch thick, and making twelve turns only round the needles, which are astatic. The deviations were determined by the torsion of a glass thread, in the way described in Chapter VIII.

338. It is surprising to those who have never before seen the experiment, with what promptitude and accuracy a copper and iron wire, soldered thus together, will indicate temperatures.

339. In the arrangement now described, when an experiment has to be made, the vessel A A is to be filled two thirds full of water, the bulb of the thermometer being so adjusted as to be in the middle of the vessel, and the soldered extremity, *s*, of the two wires being placed in contact* with it, and a small cover with suitable apertures adjusted on the top of the vessel, so that the steam, as it is generated, may rush up alongside of the tube of the thermometer, and bring the mercurial column in it to a uniform temperature. The communicating wires, *f f*, are then placed in the cups, and the trough, *e*, filled with water and pounded ice, and carefully surrounded with a flannel cloth. The

* If the extremity of the thermo-electric pair be allowed to rest on the bottom of the glass vessel, no accurate results can be obtained; the pair does not then indicate the temperature of the water.

water in the vessel A A is then gradually raised to the boiling point by means of a spirit-lamp, and kept at that temperature until the galvanometer needles and the thermometer are quite steady. The same plan must be followed when any other temperature than 212 is under trial, for the thermo-electric wires changing their temperature more rapidly than the mercury in the thermometer, it is absolutely necessary to continue the experiment for some minutes to bring both to the same state of equilibrium.

340. When a temperature higher than 212° Fah., but under a red heat, is required, I substitute, in place of the vessel A A, a tubulated retort, the tubulure of which is large enough to allow the passage of the bulb of the thermometer and the wires. A quantity of mercury sufficient to fill the retort half full is then introduced, and the tubulure being closed by appropriate pieces of soapstone, the neck of the retort is inclined upward, so that the vapour as it rises may condense and drop back again without incommoding the operator. As in the former case, it is here also necessary to continue each experiment for a few minutes, to bring the thermometer and the thermal pair to the same condition. There is not much difficulty in obtaining any required temperature, by raising or lowering the wick of the lamp.

341. The metals I have tried were in the form of wires. They were in the state found in commerce, and therefore not pure; they were obtained in the shops of Philadelphia

TABLE I.

Names of the pairs of Metals.	Temperatures (Fahr.).				
	32 F.	122 F.	212 F.	662 F.	
Copper and iron . .	0	93	176	233	Quantities of electricity.
Silver and palladium.	0	65	147	613	
Iron and palladium .	0	112	223	631	
Platina and copper .	0	11	26	122	
Iron and silver . .	0	89	137	244	
Iron and platina . .	0	28	56	248	

In this table I have estimated the temperature of boiling mercury at 662° Fahr. The quantities of electricity evolved, as estimated by the torsion of a glass thread, are ranged in columns under their corresponding temperatures. Each series of numbers is the mean of four trials, the differences of which were often imperceptible, and hardly ever amounted to more than one degree.

342. Now if this table be constructed, the temperatures being ranged along the axis of abscissas, and the quantities of electricity being represented by corresponding ordinates, we shall have results similar to those given in *fig.* 32, in which it is to be observed that the curves given by the systems of silver and iron, copper and iron, and palladium and iron, are concave to the axis of abscissas; but those given by platina and copper, silver and palladium, and platina and iron, are convex.

343. Let us now apply the numbers obtained by these several pairs for the calculation of temperatures, which will set their action in a more striking point of view. The following table contains such a calculation, on the supposition that for the 90 degrees from 32° Fahr. to 122° Fahr., the increments of electricity are proportional to the temperatures.

TABLE II.

	Temperatures by the Mercurial Thermometer.			
	32 F.	122 F.	Water boils.	Mercury boils.
Temperature by a pair of Copper and iron	32	122	202	257
Silver and palladium	32	122	235	880
Iron and palladium	32	122	211	539
Platina and copper	32	122	244	1030
Iron and silver	32	122	170	279
Iron and platina	32	122	212	829

344. We are therefore led to the general conclusion that, in *these six different systems of metals, the developments of electricity do not increase proportionally with the temperatures, but in some with greater rapidity, and in others with less.*

345. The results here given I have corroborated in a variety of ways, and with a variety of wires. A pair, consisting of copper and platina, gave for the temperature of tin, when in the act of congealing, 452° Fahr. instead of 442° Fahr., the point usually taken. For the melting point of lead, it gave 942½° Fahr., instead of 612° Fahr. The melting points of tin, lead, zinc, and occasionally of antimony and bismuth, were in this manner employed, for they allow time for the working of the torsion balance, and, with the exception of bismuth, their temperature appears to be steady all the while they are in a granular condition, before they finally solidify. The action of these metals on the thermo-electric pair is easily prevented by dipping it into a cream of pipe-clay.

346. A pair of copper and platina gave for a dull red heat 1416° Fahr., and for a bright red 2103° Fahr.

347. A pair of palladium and platina gave for a dull red 1850° Fahr., and for a bright red 2923° Fahr.

348. Some of the combinations into which iron enters as an element give rise to remarkable results; thus, if we project the curve given by a system of copper and iron, we shall find it resembling *fig.* 33, where the maximum ordinate *b* occurs at a temperature of about 650° Fahr.; the point *c* appears to be given between 700 and 800 degrees; *d* by a dull red heat; *e* is very nearly the point at which an alloy of equal parts of brass and silver melts, for if the pair be soldered with this substance, it fuses when the needles have returned almost exactly to the zero point. With harder solders, or with wires simply twisted, the curve may be traced on the opposite side of the axis towards *f*, its ordinate increasing with regularity. At 60° Fahr., taking the length of the ordinate corresponding to a temperature of 212° Fahr. as unity, the length of the maximum ordinate at *b* is 1·85, very nearly.

349. A system of silver and iron gives also a similar curve, the point *b* occurring at a temperature rather higher than the analogous one for the preceding system, but still below the boiling point of mercury.

350. Now all these things serve to show that we cannot determine with accuracy unknown temperatures by the aid of thermo-electric currents, on the supposition that the increments of the quantities of electricity are proportional to the increments of temperature throughout the range of the mercurial thermometer.

351. Let us now proceed to the second proposition, "That the tension undergoes a slight increase with increase of temperature, a phenomenon due to the increased resistance to conduction of metals when their temperature rises."

352. It will be seen, on consulting the following table, that pairs of *different metals,* at the same temperature, have tensions which are apparently very different.

353. The currents, the tensions of which are here indicated, were generated by keeping one end of the thermal pair in boiling water, the other ends being maintained at a temperature of 32° Fah.

TABLE III.

A pair of	Tension.	A pair of	Tension.
Antimony and bismuth .	·137	Platina and iron	·470
Copper and iron	·183	Copper and platina . . .	·473
Silver and lead	·307	Platina and palladium .	·500
Lead and palladium . . .	·313	Tin and iron	·518
Silver and platina	·380	Platina and tin	·567

We perceive, therefore, that there apparently exist specific differences in the qualities of electric currents derived from different sources. If, for example, we take a pair of platina and palladium, and expose it to a temperature which shall generate a current capable of deflecting the torsion balance through 1000 degrees, and then obstruct it by a wire of such dimensions as to stop one half, or only allow 500 degrees to pass, and repeat the experiment with a current generated by bismuth and antimony, the temperature being still so adjusted as to give a deflection of 1000 degrees, on making this pass through the same intercepting wire, perhaps not much more than one eighth of it will go through the galvanometer.

354. It might be supposed that these characteristic differences of thermal currents, derived from different sources, were due to some modification of the electricity itself, similar to those of radiant heat derived from different sources, or at different temperatures, which M. Melloni has attempted to show are analogous to the colours of light, being, like them, of different degrees of refrangibility, and permeating absorbent media with different degrees of facility. For in the same way that we regard glass as transparent to light, and rock salt as transparent to heat, so, too, we might regard a copper wire or any conducting medium as transparent to electricity.

355. But this peculiarity of thermo-electric currents depends on the conducting resistance of the system that generates them. It is possible to give a current a higher or a lower tension, by simply making use of thin or thick wires to generate or to carry it. In the foregoing table, the current from platina and palladium had a high tension, because slender wires of those metals happened to be used to generate it; and the current from antimony and bismuth had a low tension, because thick bars of those substances were employed. In the former case, the conducting resistance was greater than in the latter, and hence the tension of the current was higher.

356. That this is strictly true will appear on examining the current evolved by any number of systems under the same condition of resistance to condition. I took a pair of copper and iron, and soldered it to a simple pair of platina and copper, as is shown in *fig.* 34, so as to form one continuous metallic line. The point of junction formed by the wires i (iron) and p (platina) was kept carefully at 63° Fah., by immersion in a water-bath; the point of junction, p (platina) and c (copper), was treated in like manner, but that of c and i was raised to 212° Fah. Under these circumstances, it was found that 181 degrees of electricity were evolved, of which fifty went through a given

secondary wire. Then, raising the junction *p* and *c* to 212° Fah., and bringing *e* and *i* to 63° Fah., there passed at the galvanometer 71 degrees, of which 19 could traverse the same secondary wire, but

As 181 : 50 : : 71 : 19·6;

and hence I infer that, where the conducting resistance is the same, the tension of currents from different sources does not differ.

357. These results inform us how much the tension of a current depends on the resistance to conduction of the system which it traverses, as well as on the dimensions of the system itself; an observation the value of which we shall presently see.

358. In a great number of trials which I made, I failed in getting any trustworthy results as respects tension of currents at high temperatures, on account of the difficulty of maintaining the thermo-electric pair at the same degree without variation. By employing, however, a small blacklead furnace, to which was adapted a covered sand-bath, into which the wires could be plunged, I succeeded at last; for with this arrangement a regulated temperature could be kept up for a length of time.

359. The experiment was made with care in the case of two systems of metals: 1st, copper and platina; 2d, copper and iron.

1st. At the boiling point of water, a pair of copper and platina, the unexcited extremity of which was carefully maintained at 67° Fah., evolved as a mean of four trials, three of which were absolutely identical, 123 degrees of electricity, of which 23 could pass a secondary wire.

Then, by the aid of the furnace and sand-bath, the temperature was raised until the pair evolved 783 degrees as a mean of four trials; of these 163 could pass the secondary wire. Now, As 783 : 163 : : 123 : 25½ instead of 23,

showing, therefore, a slight rise of tension.

360. 2d. The pair of *copper* and *iron* gave, at the boiling point of water, 300 degrees, of which 57 passed the secondary wire. The temperature was now raised, with the following results:

490	degrees	passing	the primary,	95	the secondary	wire.
553	"	"	"	113	"	"
545	"	"	"	112	"	"
493	"	"	"	110	"	"

It will be understood, that although the quantities of electricity indicated in the first column do not regularly increase, the temperatures were, notwithstanding, going regularly upward; to this peculiarity of the systems into which iron enters I have already alluded (348). Let us now compare these measures with those obtained for the boiling point of water:

As 490 : 95 : : 300 : 58 instead of 57.
553 : 113 : : 300 : 61 "
545 : 112 : : 300 : 61 "
493 : 110 : : 300 : 67 "

We find, therefore, that in the case of both these systems of metals, the tension slowly rises with increase of temperature, being much better marked in the latter than in the former instance.

361. The increase of tension here detected depends unquestionably on increased resistance to conduction, which the wires exhibit as their temperature rises, as the following experiments show.

362. A pair of copper and iron evolved a current at the boiling point of water, which, passing through a wire of copper eight feet long, was determined at the galvanometer to be 176 degrees. Having twisted a part of this wire into a spiral, so as to go over the flame of a spirit-lamp, 8 inches of it were thereby brought to a red heat; the deviation of the needle fell now to 165, being a deficit of 11 degrees. In this experiment, care was taken that no heat should be transmitted along the wire to the connecting cups.

363. The same was repeated with a piece of iron wire, of the same length and under the same circumstances. The current at first being 90 degrees, as soon as the spiral was made red hot, it fell to 61 degrees, being a deficit, therefore, of nearly one third the whole amount.

364. To the increased resistance to conduction, occasioned by an increased temperature, we are to impute the slight rise of tension observed in thermo-electric currents. The quantities are of the same order.

365. We have next to show "that the quantity of electricity evolved at any given temperature is independent of the amount of heated surface; a mere point being just as efficacious as an indefinitely extended surface."

366. The quantities of electricity evolved by hydro-electric pairs has been shown to increase with their surfaces, but it is not so in thermo-electric arrangements. A pair of disks of copper and iron two inches in diameter were soldered together; they had continuous straps projecting from them, which served to connect them with the galvanometer cups. At the boiling point of water they gave 62 degrees; on being cut down to half an inch in diameter, they still gave 62. On the disk being entirely removed, and the copper made to touch the iron by a mere point, its extremity being roughly sharpened, the deflection was still 62.

367. By means of a common deflecting multiplier, I obtained the following results: 1st. A copper wire being placed in a bath of mercury, the temperature of which was 240° Fah., I dipped into it a second copper wire, the temperature of which was about 60° Fah.; the galvanometer needles moved through 15 degrees.

2d. The cold wire being sharpened to a point, and plunged deliberately into the mercury to the bottom of the bath, the deflection was 19 degrees.

3d. But when I touched the surface of the mercury with the *very point* of the cold wire, there was a deflection of 60 degrees.

368. Having laid a plate of tinned iron upon the surface of some hot mercury, it was touched with the point of the cold wire. There was a strong deflection of the needles in the opposite direction to what would have been the case had the mercury been touched, and not the iron. The under surface of the iron was therefore acting as a hot face, and the parts round the point as a cold face, being temporarily chilled by the touch of the wire.

369. These results explain the anomalies observed by some of those who investigated the course of thermo-electric currents by means of small metallic fragments.

370. It would therefore seem that, when wires of the same metal are used as electromotors, the quantity of electricity evolved depends on the quantity of caloric that can be communicated in a given time. Time, therefore, under these circumstances, must enter as an element of thermo-electric action. In the case of a single metal, the maximum effect would be produced when the hot element is a mass, and the cold one a point.

371. And, lastly, "That the quantities of electricity evolved in a pile of pairs are directly proportional to the number of the elements."

372. In the first trials which I made to determine the effect of increasing the number of pairs in a pile, the results obtained were contradictory; by operating, however, in the following way, the proposition was at last satisfactorily determined:

1st. The resistance to conduction was made nearly constant by uniting all the pairs intended to be worked with at once. The current, therefore, whether generated by one, two, three, four, or more pairs, had always to run through the same length of wire, and experienced in all cases a uniform resistance.

2dly. By making each individual element of considerable length, the liability of transmission from the hot to the cold extremity was diminished.

373. Having, therefore, taken six pairs of copper and iron wires, one sixteenth of an inch thick, and each element 38 inches long, I formed them, by soldering their alternate ends, into a continuous battery. Then I successively immersed in boiling water one, two, three, &c., of the extremities, their length allowing freedom of motion, and the other extremities not differing perceptibly from the temperature of the room.

374. The following table exhibits one of this series of experiments:

TABLE IV.

No. of Pairs.	Calculated Deviations.	Observed Deviations.
1.	55	55
2.	110	111
3.	165	165
4.	220	220
5.	275	272
6.	330	332

Hence there cannot be any doubt that the quantities of electricity evolved by compound batteries, at the same temperature, are directly proportional to the number of the pairs.

375. With some general remarks, arising from the foregoing subjects, I shall conclude this chapter.

376. It is of importance to remember that thermo-electric currents traverse metallic masses only on account of differences of temperature existing at different points.

377. When a current of electricity, flowing from the poles of a battery, is made to traverse a metallic sheet, the whole of it does not pass in a straight line from one pole to the other, but diffuses itself through the metal, diverging from one point and converging to the other. The greater part of the current is found, however, to take the shortest route.

378. Combining, therefore, the foregoing observations, we perceive that there are certain forms of construction which will give to thermo-electric arrangements peculiar advantages. For example, the surfaces united by soldering must not be too massive. Let

a, fig. 35, oe a bar of antimony, and *b* a bar of bismuth ; let them be soldered together along the line *c d*, and at the point *d* let the temperature be raised ; a current is immediately excited ; but this does not pass round the bars *a*, *b*, inasmuch as it finds a shorter and readier channel through the metals, between *c* and *d*, circulating, therefore, as indicated by the arrows. Nor will the whole current pass round the bars until the temperature of the soldered surface has become uniform.

379. An obvious improvement in such a combination is shown in *fig.* 36, which consists of the former arrangement cut out along the dotted lines : here the whole current, so soon as it exists, is forced to pass along the bars ; and because the mass of metal has been diminished at the line of junction, such a pair will change its temperature very quickly.

380. One of the very best forms for a thermo-electric couple is given in *fig.* 37, where *a* is a semi-cylindrical bar of antimony, *b* one of bismuth, united together by the opposite corners of a lozenge-shaped piece of copper, *c*. From its exposing so much surface, the copper becomes hot and cold with the greatest promptitude, and, from its good conducting power, it may be made very thin without injury to the current. With a pair of bars three fourths of an inch thick, and a circular copper plate, *c*, having both surfaces blackened, I have repeated the greater part of those experiments which M. Melloni made with his multiplier.

381. The currents which circulate in a steel magnet are to all appearance perpetual. I thought for some time it might be possible to procure similar perpetual currents by compound thermo-electric arrangements. *Fig.* 38 will serve to show the character of these combinations, and also the cause of their failure. Let *a*, *b*, *c*, be wires of three different metals, soldered together so as to form a triangle. Now, if these metals were selected, so that *a* and *b* could form a more powerful thermo-electric pair than *a* and *c* or *b* and *c*, it might be expected that at all temperatures an incessant current would run round the system. Such, however, will not be found to be the case. In effect, any one of these three serves simply as a connecting solder to the other two, and hence no current is excited ; for the ends that have the third metal between them, although that metal intervenes, are under exactly the same condition as the other ends which are in contact.

382. Thermo-electric currents, evolved by pairs of different metals, do not appear to differ specifically. As different gases during combustion burn with differently-coloured flames, and as different sources of caloric evolve rays of heat which are absorbed differently by different media, it might be expected that a pair of wires of copper and platina would give out a current of electricity unlike that of iron and palladium. I have made many trials on this point, adjusting a wire of copper and one of lead to each other, so as to stop equal quantities of electricity flowing from a pair of copper and platina, the galvanometer needles being brought to the same point, whether the long wire of copper or the short wire of lead was employed. But, in the case of every combination which I tried, these two wires acted alike, nor could I ever evolve a current which would pass with more or less resistance along the lead than along the copper.

CHAPTER X.

EXPERIMENTS ON THE CHEMICAL ACTION OF SOLAR LIGHT.

(*From the Journal of the Franklin Institute for June, July, August, and September,* 1837.)

CONTENTS: *Action of Absorbent Media.—Ideal Colouration of the Chemical Rays.—Specific Absorption.—Colorific Absorption.—Calorific Absorption.—Specific Absorption of the Chemical Rays.—Effect of Yellow Solutions.*

Decomposition of Carbonic Acid by Leaves.—Penetration of Dimensions in Gases.—Decomposition of Carbonic Acid under various coloured Media.—Gas from Leaves contains Nitrogen.—Chemical Rays of different Colours.—Identity of Volume in the absorbed and evolved Gas.—Cause of the Decomposition.

Ritter's Experiments of the Non-oxygenation of Phosphorus.

Decomposition of the Salts of Silver.—Prismatic Spectrum on Bromide of Silver.—Interference of Chemical Rays.—Salts decomposed by Light.—Moonlight and Artificial Flames are Inactive.

Of Perihelion Motions.—Dew of Water and Mercury.—Iodine.—Chloride of Gold.—Non-deposition on a Glass Plate.—Current Action.—Action of Flame.—Action of Metal Screens.—Protecting Action of a Metal Ring.—Is there Electricity in the Solar Ray?

Cause of the Green Colour of Leaves.—Plants grow in Lights of various Colours.—Seeds also germinate in Red, Yellow, and Blue Light.—Chemical Rays of different Colours.

383. THE effect of absorbent media upon the colorific rays of light has been, as was predicted by an eminent writer on optics, of singular service in developing new views of this subtle agent, and giving us a more precise knowledge of the complex constitution of the solar beam. Hitherto, the action of these media upon the calorific and chemical rays has not been thoroughly investigated, nor are there, so far as I know, any experiments on record exhibiting this matter in its full importance.

384. We have been accustomed to regard the chemical properties of the solar spectrum as due to the violet ray. A similar opinion was formerly maintained respecting the calorific constitution of the red ray. The position to which we are brought by advanced investigation has long ago established the separate existence of heat-making rays, and the experiments here communicated give much weight to the doctrine that the chemical rays have also a separate existence. It is true it cannot yet be proved, though analogy and probability are favourable to the idea that there are subdivisions both of the chemical and calorific rays, similar to those of which our senses give evidence in the colorific ray, each of which is endued with distinct powers of its own.

385. How complex is, then, the constitution of the solar beam! a ray of heat, composed, perhaps, of three or more rays of different refrangibility; a ray of light, composed

of three simpler rays; a ray endued with chemical energy, and of a similar composition to the former, as analogy would lead us to suspect. Again, each of these elementary rays is composed of particles, one half of which have their planes of polarization at right angles to the other. All these elements taken together constitute a beam of the sun's light. Emanations from the sun, after they have undergone the absorptive action of the atmosphere of that great luminary, and of that of the earth, still reach us in abundance, accompanying his light, and traversing the great vacuum, perhaps as far as his attraction is felt.

386. If we take a coloured medium of any kind, and transmit through it a beam of the sun's light, we find, on examination, that certain of the rays exciting vision are absorbed, that the light which passes through is not homogeneous, for it is capable of decomposition by the prism; it is a compound coloured ray, consisting of all the rays, complementary to those which the medium has absorbed. Nor is this absorbing effect confined to the rays producing vision; the rays of heat suffer in like manner; sometimes those which are more refrangible are wanting, sometimes those which are of less, or of medium refrangibility, are absent. Often, at the same time, do the chemical rays sustain a similar attack. There are solutions and media transparent to light, and nearly opaque to heat; there are others transparent to light and to heat, and opaque to the chemical ray. It is from these facts that we are able to establish the separate existence of three genera of rays in the sunbeam, each of which is essentially distinct in its properties, and different in its mode of action from the others. Our eye can detect, in the rays exciting vision, difference of constitution, because we are able to perceive a difference of colour. Had we specific organs for indicating difference in the heat-making, or chemical rays, perhaps we might find in them a similar constitution.

387. It is between three and four years since that the investigation which forms the subject of this memoir was first commenced, under the form of an examination of the properties of the chemical ray. In the Journal of the Franklin Institute some of the earlier results are recorded, and among them the fact, that the crystallization of camphor, which has long been known to take place on the enlightened sides of vessels exposed to the sun, occurs with very great rapidity, if the glass in which it is tried be exhausted of air. In tracing out this fact, to ascertain its cause, a field of no common interest has been entered. I do not here present a record of the facts as they were successively developed by an analysis of the phenomena, but place them in that order which appears to me the best to obtain a true estimate of their bearing.

388. Into a darkened chamber, the shutter of which is seen in section at *a a*, *fig.* 39, a beam of the sun's light may be made to pass horizontally, by means of a mirror of silvered glass, *c*. The mirror which I use is one belonging to a solar microscope, and by turning the milled screws, *e e*, it can be brought into any position required to throw a beam horizontally into the room, no matter what may be the place of the sun. A brass tube, *f*, belonging to the same instrument, and two inches in diameter, can be screwed into the position figured, if desirable; there is also a lens, *g*, which may occasionally be fixed at *g*; its focus is nine inches, its diameter about two inches, and the diameter of the sun's image $\frac{1}{10}$ of an inch.

389. A piece of sheet lead, about a quarter of an inch thick, is to be cut into the form of a horse-shoe, of such magnitude that a circle one inch in diameter might be inscribed in it. Upon this lead two pieces of very pure and transparent crown glass are cemented, so as to form a trough, for containing a variety of liquids. It is well to accommodate this trough with a strong foot, or basis, *a a*, and several such troughs may be provided. *Fig.* 40, *c c c*, the leaden horse-shoe; *b b*, the glass plates.

390. A thin metallic plate, three or four inches square, having a longitudinal slit about one inch long and $\frac{1}{16}$ inch wide in it, is also to be provided. It is convenient that this, too, should be furnished with a pediment. *Fig.* 41, *a a*, the slit.

391. The lens (*g*, *fig.* 39) having been removed, by turning the screws a beam of light is to be thrown horizontally into the room; the screen (*fig.* 41) is then to be placed before the brass tube, *f*, so that the slit in it may allow a narrow streak of light to pass. The trough (*fig.* 40) is then placed behind, in such a position that half the light which comes through the slit in the screen may pass through the liquid contained in the trough, and the other half pass by its side unintercepted. This arrangement is shown in *fig.* 42. Behind the trough is placed a flint glass prism, *d*, *fig.* 43, and, farther still, a white pasteboard screen, *e*, of suitable dimensions, *a* being the metallic screen, *b* the trough.

392. The action of this arrangement is as follows: the beam of light cast by the mirror into the room is entirely intercepted, except the small portion which passed through the slit in the metallic screen. A part of this passes through the trough, and a part on one side of it, the middle part being obstructed by the leaden horseshoe. Two beams of light, therefore, fall on the prism, one of which has passed through the trough, and one which has not, and they are separated from each other by a dark interval. The prism disperses both, and there fall on the pasteboard screen two spectra, side by side, close enough for a very accurate examination. One of them has been acted on by the fluid in the trough, the other is undisturbed. In my arrangement, the spectrum *a* happens to be the natural one, and *b* the disturbed one (*fig.* 44).

393. Let us now take an example, as an illustration of the use of this apparatus. Fill the trough with distilled water, and let the mirror throw a horizontal beam. Two spectra are seen on the screen, *e*, *fig.* 43, close to each other, side by side, with a dark interval between them. They contain, as may be perceived, all the seven colours of Newton, nor does the one differ in any wise from the other, as in *fig.* 45.

394. Having poured the water out of the trough, fill it with a strong, but clear solution of the chromate of potassa; on looking at the spectra on the screen, *a* is still found of its natural appearance, but by the side of it there is a distorted spectrum, formed by the light that has passed through the trough; the blue, the indigo, and the violet rays are wanting, as is seen in *fig.* 46. These colours have then been absorbed by the solution of chromate of potassa. If this solution be poured out, and one of sulphate of copper and ammonia poured into the trough, another kind of spectrum is produced, where the red and much of the yellow light is wanting, as in *fig.* 47. If a strong solution of Brazil wood is used, the disturbed spectrum will be found to have lost its violet, indigo, blue, green, yellow, and a great part of its orange rays, as represented in *fig.* 48.

395. By having the two spectra side by side, and close to one another, they are placed under circumstances most convenient for making a perfect comparative estimate of the light which is lost. In this manner the following table has been constructed. The specific gravity of the solutions is not given, as it is not supposed that any direct connexion exists between the density of a solution and its absorptive power. Much more depends on the shade of colour.

TABLE OF COLORIFIC RAYS ABSORBED BY SOLUTIONS.

Name.	Rays absorbed.
Bichromate of potassa	blue, violet.
Prussiate of potassa	extreme red, extreme violet, yellow.
Sulphate of copper	extreme red.
Chloride of gold	violet.
Chloride of platinum	extreme violet.
Sulp. copper and ammonia	red, yellow.
Solution of tannin	violet, indigo blue, orange, and a part of green.
Solution of litmus	orange, yellow, green, extreme violet.
Chromate of potassa	extreme red, blue, violet.
Linseed oil	violet, indigo blue.
Hydro-sulphate of lime	violet, blue.
Decoc. logwood in alum-water . . .	orange, yellow; blue, and green.
Decoc. of Brazil wood	violet, indigo blue, green, yellow, orange.
Cochineal in cream of tartar solution .	yellow and part blue.

396. Some remarkable phenomena may be produced by taking double solutions; a beam which has passed through a stratum of solution of sulphate of copper and ammonia, and then through a decoction of Brazil wood, becomes almost totally extinct. On looking through such solutions separately at the noontide sun, he appears with overpowering effulgence, but on using them together, only a very faint trace of a dirty olive-green light indicates his position. The sulphate of copper and ammonia absorbs the red rays, and the Brazil-wood decoction nearly all the remainder.

397. Already have some of these phenomena of absorption, in the hands of Sir D. Brewster, disclosed important facts respecting the colorific rays. The colour of the sky, and of the clouds, and of the sea, has also been long attributed to an action of this kind, exercised by thick masses of air, or vapour, or water.

398. But this action is not alone confined to the rays producing vision, it extends to the other elementary constituents of the spectrum. While the trough (*b, fig.* 43) is filled with a solution of sulphate of copper and ammonia, if the prism and the metallic screen be removed, and a very delicate thermometer be plunged in the ray, a new phenomenon is discovered; the ray is found to be, to a great extent, deprived of the power of exciting heat, and the thermometer shows little disposition to rise. How is this! is it because the red ray is gone that the sunbeam has lost its power of exciting a sensation of warmth? It was at one time supposed that as the violet ray had the power of determining chemical change, so the red ray possessed the power of exciting calorific impressions.

399. Fill the trough next with a strong decoction of Brazil wood, analyze the light which passes through it by the prism (391), and it will be found that all the rays have been absorbed except the red. Now, in such a beam, if the red ray possess inherent caloric, the thermometer should rise as much, or nearly as much, as if it were in the direct solar ray; if the colour passes unabsorbed, so too should the caloric; but, place the thermometer in the beam, and it does not rise. Or throw a concentrated column

of such light upon it by a convex lens, and it is still unmoved. We are therefore forced to conclude that the rays exciting heat are independent of those exciting vision; that neither the red, nor the yellow, nor the blue possesses inherent caloric; and, moreover, that substances may be transparent to red, to yellow, or to blue light, or to all, and yet more or less opaque to the rays of heat.

400. It is not alone among watery solutions or alcoholic tinctures that we find abundant instances of this kind of action; the mineral kingdom furnishes many. A very thin lamina of pitch is transparent to red light, but almost opaque to the rays of heat. I have examined a variety of bodies, gaseous, liquid, and solid, and shall here point out the method which has been followed in obtaining the results contained in the following table.

401. The mirror being placed upon the shutter, as in (388), a plano-convex lens is to be screwed into the tube, so as to bring the rays to a focus on one of the balls of a very delicate differential thermometer; the motion of the fluid is rapid, and the instrument soon attains a position of equilibrium: this gives the heat of the sunbeam as concentrated by the lens. To find the effect of any liquid medium in absorbing these rays, the trough filled with the substance under trial is placed at the extremity of the brass tube, in a position as at *c* (*fig.* 50). The cone of rays converges from the lens, *a*, on the ball *b*; but because the trough has plain and parallel surfaces, the rays still pass on, and form an image on the focal ball of the thermometer. The total effect, as given by the expansion of the air in the instrument, and which has formed the basis of the following table, is not, however, an exact estimate of the action of the liquid solution. In the instrument which I am in the habit of using, the convex lens is of flint glass, and the plates of the trough of Boston crown glass; there are, therefore, at least two disturbances, the absorbing action of the former, and still more powerful effect of the latter. It has been considered, from the experiments of Melloni, that the power of absorption was inversely as the power of refraction, but whether an extended train of investigations will corroborate this supposition, remains to be seen. In the following experiments, the instrumental arrangement being always identical, a comparison may be instituted of the action of any two of the solutions; but the absolute action of each cannot be determined, except after allowing for the additional effect of the flint glass lens and the crown glass plates. In practice, it will be found very useful to blacken the focal ball of the thermometer, as seen at *b* (*fig.* 50). It serves to give a larger scale of thermometric expansion. It is also requisite to cover the thermometer with a very thin case of pure and transparent glass, which prevents the disturbance of currents, and also of the heat radiated from other bodies in the vicinity; this introduces, however, the absorptive action of a third plate of glass; *b d* is the thermometer, and *e e* the glass cover (*fig.* 50).

402. By these arrangements, it was found that a thin stratum of pitch enclosed between two plates of crown glass, and which transmitted a homogeneous red light, absorbing all the other colours of the spectrum, allowed only nineteen rays of heat to pass through of every hundred that fell upon it.

403. A solution of the sulphate of copper and ammonia, which absorbs the red and

the yellow light, being operated upon in like manner, was found to transmit twenty rays out of every hundred that fell upon it.

404. There is, however, considerable difficulty in obtaining these numerical results with accuracy, arising partly from the difficulty of obtaining specimens of exactly the same composition, but more especially owing to changes taking place in their colour. In process of time, most vegetable solutions undergo spontaneous changes, and no longer give the same results. But, where the same sample is operated on, under the same circumstances, repeated experiment assures me that this arrangement gives comparable indications.

405. Vapours and gases may also be put under trial. The vapour of iodine, the spectrum of which is remarkable as containing only the extreme rays, and wanting those of medium refrangibility (*fig.* 49), absorbs two thirds of the heat that impinges on it. The vapour of nitrous acid, which stops the violet, blue, indigo, and yellow light (*fig.* 51), has a similar effect on the heat. To experiment upon these bodies, a cubical bottle (*fig.* 52) is very convenient to generate the vapour in, and also to transmit the light through; it will then replace the trough of (389). Nitrous acid vapour is best made for these purposes from nitrate of lead.

406. Having prepared a variety of solutions for the purpose of experiment, and using for each the same trough, thoroughly cleansed after each trial, the following table will give an estimate of the results obtained; it is arranged according to the power of each solution, the first on the list being the most energetic:

TABLE OF THE THERMO-ABSORPTIVE POWER OF SOLUTIONS.

Decoction of logwood in alum water,	Bichromate of potassa,
Solution of sulphate of copper and ammonia,	Hydro-sulphate of lime,
Litmus water,	Muriate of iron,
Decoction of Brazil wood,	Oil of turpentine,
Decoction of cochineal,	Prussiate of potassa,
Solution of tannin,	Sulphate of copper,
Solution of chloride of chromium,	Chloride of platinum,
Tincture of turmeric,	Chloride of gold,
Tincture of saffron,	Oil of bergamot,
Sulpho-cyanate of iron,	Linseed oil,
Hydro-sulphate of ammonia,	Nitrous ether,
Muriate of cobalt,	Water.

407. Still more powerful effects are produced by making binary or ternary arrangements. If, for instance, a beam of the sun falls upon a very thin, transparent stratum of pitch, and then passes through a solution of sulphate of copper and ammonia, or through linseed oil, not more than one fortieth part of the caloric is transmitted.

408. A question here naturally arises: What becomes of the heat thus lost? does it enter into such a combination with these media as to be detected in them by the thermometer as sensible heat? This is a question of much difficulty; there are so many disturbing causes in operation, the general results of experiment have not yet furnished actual proof that the heat missing is to be found in the fluid solutions. I would not, however, be understood to deny that such is the case; only that, at present, our information does not warrant such a conclusion. It might be supposed that these solutions do not act by a proper absorptive power, but merely offer that kind of obstacle to the transmission of heat that turbid media do to light. Not only, however, does

direct experiment discountenance this, but the analogy of their action on the chemical ray renders it extremely improbable, an action which I proceed to develop, *fig.* 50, being still consulted.

409. Having removed the differential thermometer and its case, and produced a cone of light converging from the lens, and passed it through a solution of sulphate of copper and ammonia contained in the trough, if now we hold in the focus a piece of bibulous paper imbued with chloride of silver, although little or no heat is transmitted through the solution, yet an extremely dark spot is produced, characteristic of the blackening of that substance by the solar rays. Though, therefore, the double salt transmits the ray of heat with difficulty, the ray of chemical action passes with great facility. If a trough containing a strong solution of bichromate of potassa be now substituted, a far larger quantity of light will pass, and vastly more heat; but a paper imbued with chloride of silver being laid in the focus, *no chemical change whatever goes on*, the chloride retaining its usual whiteness.

410. I placed a piece of paper imbued with chloride of silver in a cubical box, one of the sides of which was formed of a pair of glass plates, with a solution of bichromate of potassa between them; it was exposed for many days to the sun's light, and only assumed a faint bluish stain, while a similar piece exposed to the direct rays was fully blackened in fifteen minutes. So powerful is the action of this salt, that when a stratum of it not more than the hundredth part of an inch thick was included between two plate glasses, it stopped the decomposition of chloride of silver. It was after a long examination of a variety of substances that I first became acquainted with the great absorptive power of the chromates of potassa. In my earlier experiments, I had made use of the chloride of platinum and the chloride of gold, both of which have an analogous action. The solutions which I have recognised as possessing this power in the most eminent degree are,

Bichromate of potassa.	Muriate of iron.
Chromate of potassa.	Chloride of gold.
Yellow hydro-sulphuret of ammonia.	Chloride of platinum.
Hydro-sulphate of lime.	Coloured vegetable solutions.

It is remarkable that all the *mineral* solutions on this list are *yellow;* the absorptive power, however, is by no means connected with that colour, for the yellow tint is a compound one; all the rays of homogeneous light are absorbed by one or other of the bodies on the foregoing list.

411. It is interesting to know whether these absorptions be really the abstraction of something from the solar ray, or merely some change impressed upon it. If light consists of tremblings, pulses, or undulations, or any other kind of motion of a homogeneous elastic medium, in virtue of which it is competent to excite sensations of heat and effect chemical changes, we might explain the action of these media as the result of some change occurring to that motion, either in direction or degree. We might suppose, too, that when a ray had been deprived of its power by passage through one medium, it might have it restored, in a greater or less degree, by being transmitted through another. I have not found, in thus comparing together nearly three hundred

media, any indications of such a result, and therefore suppose that something of a material character has been abstracted from the ray; that it is really a loss, and not a change.

412. Being thus possessed of the means of depriving the beams of the sun of their heat and their chemical force, I have proceeded to examine a variety of questions of interest. A great many changes in the constitution of bodies, on their exposure to light, are recorded in the books of chemistry and physics, but they are there imputed to light *in the aggregate*, without any reference to its compound character: we shall find there are changes due to the colorific ray, changes due to the calorific ray, and changes due to the chemical ray.

413. Decomposition of Carbonic Acid by Leaves.—One of the most important and extensive functions exercised by radiant matter from the sun is the decomposition of carbonic acid by vegetable leaves, and the elimination of oxygen gas. Vegetable physiology looks to chemistry for information, but hitherto the chemist has not possessed the means of perfectly developing the matter. Its intrinsic importance entitling it to investigation, I shall not offer any apology for passing from the direct object of this paper to the mention of some facts necessary to the thorough understanding of the matter.

414. It would appear that there is a particular kind of combination, to which attention has hardly yet been drawn, distinct from what is understood by chemical combination and mechanical mixture. A pint of alcohol and a pint of water being mixed together, the result will measure somewhat less than a quart, and the same might be indicated of a variety of other liquids. No instance, I believe, is yet on record of a like penetration of dimensions being observed in the case of gases; if it exist at all, it exists to a very small amount, and the change of volume which these bodies readily experience, by alteration of temperature and pressure, renders so minute an effect very difficult of detection. It has been supposed, judging from analogy, that the constituent gases of the atmosphere, the uniting volume of which is always constant, are held together in this manner, or that the whole volume is condensed and retained by some force of compression. There are some experiments which indirectly prove this: sound passes along different media with different velocities; if a cannon were therefore discharged at a distance, it should impress the ear with two distinct sounds; the one coming along the particles of nitrogen, and the other along the particles of oxygen. But it is well known, from observations made directly on this point, that instead of there being any reduplication of the sound, it comes clear, distinct, and alone; we have, therefore, to infer that these two gases are held together in a state of compression.

415. In making experimental investigations on this matter, two different courses may be followed: first, we may measure the resulting volume after the mixture of known volumes of the gases under trial; or, secondly, we may ascertain whether any thermal disturbance takes place during the act of their uniting; the latter is the mode I have followed in my researches.

416. Take a cylindrical glass (A, *fig.* 53) two inches in diameter and four in height; close its upper extremity with a flat piece of wood by means of cement; in the centre

of it cement a stopcock, *a*, of large bore; and, at a suitable distance from that centre, make two holes, the one to have a piece of bent tube, *b*, cemented into it to serve as a gauge, the other to have a piece of copper wire, *c*, bent into the shape (*c*, *fig.* 54), passed through it air-tight, by means of a cork, *x*, imbued with tallow. The other extremity of the cylindrical glass is likewise to be closed by a flat piece of wood larger than the former, for the purpose of bearing a little cup, *d*, containing coloured water, into which the gauge tube may dip, and in its centre it is to be perforated to admit of an arrangement, as in *fig.* 55, where *d* is a perforated cupping-glass, having a stopcock, *b*, mounted on it, the farther extremity of which opens into a glass pipe, *c*, which terminates in a hole, in the centre of a flat copper circle, *a*, three quarters of an inch in diameter: this arrangement is to be cemented, air-tight, into the flat piece of wood that closes the lower extremity of the cylindrical glass, as is seen in *fig.* 53. Moreover, beneath the cupping-glass there is a glass reservoir, *g*, of suitable dimensions, filled with water. The object of this arrangement is to fill a soap bubble with any gas, to expose it to atmospheric air, to burst it at pleasure, and to mark any thermal expansion of the two gases by the indications of the gauge; the mode in which this is accomplished will be described in the following illustration.

417. The whole apparatus having stood for some time in a quiet room, along with the gases to be tried, until they have all acquired a uniform temperature, close the lower cock, fill the cupping-glass with hydrogen gas, and raise the reservoir, *g*, so that the level of the water may be near the top of the cupping-glass. The upper cock, *a*, being open, convey through its bore, by means of a glass tube of smaller diameter, a little soap-water, which is to be deposited on the copper circle in its centre, over where the glass pipe, *e*, opens; the tube is then withdrawn. Next open slowly the lower cock, and as the gas is expelled from the cupping-glass by the pressure of the water in the reservoir, it expands a bubble in the large cylinder, the displaced atmospheric air passing out through the upper cock. When this bubble has attained the dimensions desired, close both cocks, and observe if the liquid in the gauge be stationary; if so, turn the wire *c* on its axis, so as to bring its crooked extremity, which is within the cylinder, in contact with the bubble; it bursts, there is a thermal disturbance, and an expansion of the two gases, for the fluid in the gauge instantly falls, and as the gases cool, it slowly returns to its former position. If a bubble of atmospheric air be employed instead of a bubble of hydrogen, these effects will not ensue. We therefore conclude, that when hydrogen gas is mixed with atmospheric air, the temperature suddenly rises, and therefore that it is probable that the volume of the mixture is less than the sum of the volume of its integrant constituents.

418. If a soap bubble, filled with hydrogen, be burst in an atmosphere of nitrogen gas, which may be effected by using a more complex arrangement than that indicated in the preceding section, there is also a thermal expansion, indicating that the constituents of ammoniacal gas, even without chemically uniting one with another, exercise an attraction for, or a pressure on, each other, a kind of capillary action. These compounds, for they form a distinct class of bodies, a class by no means of small extent, require a distinct name. I have suggested that of capillary compounds, because they

exist under, and can be separated by, the force of capillary attraction. An example will here illustrate what is meant. Oxygen and hydrogen gases may be mingled with each other in the proportion of one to two, the result existing in a compressed state, and forming a capillary compound; the contact of flame, or the passage of an electric spark, changes it into aqueous gas, a chemical compound; in the former state, decomposition is readily effected by capillary attraction; in the latter it cannot produce such a result. The general law of these decompositions by tissues without pores of sensible size is given in Chapter IV.; it is a very simple one, showing that a capillary equilibrium is gained only when the composition of gaseous media on each side of a barrier is chemically the same. This was proved by exposing extremely thin soap bubbles, filled with different gases, to different gaseous atmospheres, and then measuring and analyzing the media within the bubble and without. This law is applicable not only where a barrier separates a gas from a gas, but also when one of the gases is held in solution by water; and in the energy with which the media endeavour to attain an equilibrium, is to be found not only one of the causes of the decomposition of carbonic acid by the light of the sun, but also a very fruitful source of erroneous experimenting. Having made reference to this matter, I proceed to detail the steps which have been taken to illustrate this decomposition.

419. Take four globular vessels, such as *a* (*fig.* 56), three or four inches in diameter, with necks a couple of inches long; fill them with spring water, and put a bunch of pine leaves in it; immerse the end of the neck beneath the surface of the mercury contained in a cup, *b*. Let one of these vessels, designated A, be exposed to the sun's direct ray; a second, B, to the light which has passed through a solution of bichromate of potassa; a third, C, to the light which has passed through a solution of sulphate of copper and ammonia; and a fourth, D, in a dark place. It will be found that in the course of a few hours, A has eliminated most gas, B somewhat less, and C and D none at all; this is a very instructive experiment; we find from A and D that the sun's rays have the power of eliminating gas from solutions; from B we learn that the absence of the chemical rays does not affect the *apparent* result, but that if the calorific rays are obstructed, it ceases to go on.

420. A variety of experiments having thus convinced me that the mere evolution of gas is neither due to the rays of light, nor to the chemical rays, I have attempted to produce a like effect with the calorific rays emitted from a common fire; rays in which the light was altogether disproportioned to the heat, and the chemical power totally wanting. The arrangement is as follows: in the focus of a concave speculum of brass eighteen inches in diameter, I placed one of the glass globes of the preceding section, so that it might receive the rays emitted from a common wood fire, converged on it by the mirror. The fire was burning without flame, being what is technically called a dead fire, and the distance of the mirror eight feet. In a few moments gas was copiously liberated, more copiously than if it had even been exposed to the solar ray. In *fig.* 57, this arrangement is depicted: *a* is the concave mirror, *b* the glass matrass filled with spring water, and containing a bunch of pine leaves, *c* a cup of mercury into which its neck dips.

421. I shall have occasion to show, hereafter, that when a beam of light falls upon any surface in contact with a medium, it causes that surface to exert an apparent pressure on the medium, capable, at times, of producing singular effects; it is, therefore, probable that to this action we are to attribute the evolution of gas by vegetable leaves, spun glass, raw silk, &c. The percolation of liquids and gases through tissues, in obedience to the laws of capillary attraction, should also, on these principles, be controlled by the action of a solar beam. If we arrange two Champagne glasses (*fig.* 58), with their footstalks cut off, each capped with a thin lamina of India-rubber, their narrow apertures dipping into cups of water, so that they may be in all respects as like each other as possible, and fill them with protoxide of nitrogen, we shall find that one of them exposed to the sunbeam will throw off its gas much quicker than the other, shut up in the dark. Or, if one of them be exposed to an atmosphere much warmer than the other, the liquid confining the gas rises more rapidly. It has been remarked that the experiment of which I gave an account (52), of the passage of hydrogen gas through a thin film without pores of sensible size, is not uniformly attended with success. In examining the causes of failure, I have been able to trace them entirely to this source; at a certain temperature the effect is scarcely perceptible, but as the thermometer rises it becomes more and more marked. The same observations may be made of ammoniacal vapour. There are temperatures at which these permeations are imperceptible, but at 75° Fah. they take place with great rapidity.

422. Radiant heat, whether of the sun or of terrestrial fire, impinging on the surface of an obstacle, causes it to exert an increased action, resembling a force of attraction or pressure, on any medium with which it may be in contact. A few fibres of unspun silk being immersed in water containing the elements of atmospheric air in solution, and exposed to the sunshine, became speedily covered with bubbles of gas. The exact chemical constitution of these bubbles is determined by a variety of circumstances: by the velocity of evolution, by the solvent action of the water, which is greater for one gas than another, and by the presence or absence of the chemical rays. I shall here be excused for remarking a circumstance which appears to me indicative of a proneness, even in capillary compounds, to exhibit tendencies of combination by multiple volumes. Atmospheric air contains oxygen and nitrogen in the proportion of 1 to 4; the gas expelled from spring water contains the same elements in the proportion of 1 to 2; and the gas given off by pine leaves from water, holding carbonic acid in solution, contains the same elements in the proportion of 2 to 1.

423. The chemical rays emitted from the sun are not, therefore, the cause of the evolution of gas from liquids by fibres, or by vegetable leaves, for it takes place in their absence; the blue, the indigo, and the violet rays have nothing to do with it for the same reason; and the green, yellow, orange, and red are not the cause of it, for though they are present, it refuses to go on. To the calorific ray we are, therefore, to impute it. It happens not by the action of any kind of light operating as a mere stimulus on plants, for when the light is nearly absent, it goes on with undiminished energy.

424. The evolution of gas depending, therefore, on the rays of heat, we are next led to inquire whether the chemical rays affect the operation in any manner. To determine

this, I exposed a quantity of boiled water, which had been suffered to cool in vacuo, to carbonic acid gas, of which it absorbed a certain amount. A portion of this water was placed in the focus of the brass mirror, and was there acted upon by the non-luminous rays; its temperature never exceeded 140° Fah. In a short time the pine leaves commenced giving off gas very copiously, and continued to do so; but it was found, on trial, that nearly the whole of it was absorbed by lime-water, and that no decomposition had occurred. Therefore, though rays of non-luminous heat are competent to cause the evolving of gas, they are not able to cause decomposition.

425. The record of an analysis will place this effect in its true light: care being taken that the water should be impregnated with pure carbonic acid gas, and the leaves recent, when a sufficient quantity was evolved, 39 measures were taken, of which caustic potassa absorbed 34. Hydrogen gas being then added, a diminution to the amount of 4 volumes was produced by a platinum ball; the remaining gas proved to be nitrogen. The composition of this gas was, therefore,

Carbonic acid	34·00
Nitrogen	3·67
Oxygen	1·33
	39·00

It is proper to observe that a change very evidently takes place in the structure of the vegetable leaves, their colour becoming of a dirty brown, and their greenness is lost. Whether it is a change of their acting tissue which hinders decomposition, or whether there is some peculiarity in the constitution of non-luminous heat, which incapacitates it from producing those effects which result from caloric radiating from highly incandescent bodies, I shall proceed to discuss.

426. Let us first consider what is the action of an ordinary unchanged sunbeam on carbonic acid in solution, and in contact with vegetable matter. A wide distinction is here to be made between common spring water, such as pump water, and water charged with carbonic acid *only;* the former contains a compound of oxygen and nitrogen, isomeric with protoxide of nitrogen; but the protoxide is a chemical compound, having its two volumes of nitrogen compressed into one, while this is a capillary compound, existing with an almost insensible condensation. The processes of evolving gas from spring water and from carbonated water are essentially different; the former taking place by an exaltation of temperature occasioned by the impinging of radiant heat, no kind of decomposition at all going on; but the latter is accompanied by a true decomposition, due to the presence of vegetable matter.

427. This case will be better understood by an analysis of the gas given off from carbonated water. A certain volume of water had its carbonic acid and all other gaseous impurities expelled by long-continued boiling; it was then rapidly cooled by refrigeratory processes, and impregnated with pure carbonic acid gas; being introduced into a matrass (*fig.* 56) with a bunch of pine leaves, the neck of the matrass dipping under the surface of some mercury contained in a cup, so as to cut off communication with the atmosphere, it was exposed to the sun, the day being very favourable, clear and hot; 47·50 measures of the gas evolved were taken; a piece of caustic potassa ab-

sorbed 3·50 measures of carbonic acid, the remainder being 44·00 measures; 90 measures of hydrogen were added thereto, making the full volume 134·00 measures; a platinum ball reduced this to 67·00; indicating 22·33 of oxygen, there remaining of nitrogen 21·67. The composition of this gas was, therefore,

Oxygen	22·33
Nitrogen	21·67
Carbonic acid	3·50
	47·50

To prove that the remainder here spoken of was really nitrogen, one hundred volumes of the original gas were taken, and the electric spark passed through it; there was no diminution in the volume, nor any carbonic acid gas generated; it could not, therefore, be carbonic oxide, hydrogen gas, nor any of the carburets of hydrogen; it possessed, moreover, all the negative qualities of nitrogen.

428. But the solution was composed of carbonic acid and water, great care having been taken to cut off all access of the atmosphere during its preparation, and also during its exposure to the sun, for fear of capillary interchange of the carbonic acid with the gaseous elements of atmospheric air. None such had occurred. From what source, then, came the large amount of nitrogen gas evolved? the only elements within the matrass were carbon, hydrogen, and oxygen, yet here a large amount of nitrogen was found, which could have come from no other source than the pine leaves.

429. In this experiment the pine leaves absorbed one measure of carbonic oxide, and gave in exchange for it one measure of nitrogen, and the resulting gas contained, therefore, half its volume of nitrogen, and half of oxygen, mixed without sensible condensation.

430. Hitherto it has often been asserted by chemists, that when vegetable leaves were placed in carbonated water, they absorbed the carbon, and caused the oxygen to be evolved. Vegetable physiologists, botanists, and others, have raised a great many theories upon this fact, which, however, a long course of experiments assures me are without any foundation. There is no truth in the idea that plants absorb carbonic acid, and assimilate carbon and evolve oxygen. On the contrary, they actually evolve nitrogen, and the decomposition of carbonic acid, though remotely brought about by the action of the solar ray, is mainly due to the complex play of affinities of the elementary constituents of the plants.

431. I will here give another example in point, substantiating the same fact under different circumstances. Carbonated water that had been exposed with due care to the sun for two days being provided, 25·75 measures of the resulting gas were taken, and found to contain 1·25 of carbonic acid, for caustic potassa diminished them to 24.50. Next, 31·50 measures of hydrogen were added, making in all 56·00, and a platinum ball being introduced, there remained 7·50, indicating 16·16 volumes of oxygen and 8·34 of nitrogen, the composition of the gas being, therefore

Oxygen	16·16
Nitrogen	8·34
Carbonic acid	1·25
	25·75

Allowing for unavoidable errors of manipulation, the resulting gas was, therefore, one third nitrogen and two thirds oxygen, united without sensible condensation.

432. If any farther proof was required that the evolution of nitrogen by the plant is an essential part of this decomposition, it is furnished by the results of an experiment in which spun glass was used to replace the pine leaves. This arrangement, though exposed to the sun under the most favourable circumstances, will not evolve any gas, but on passing into it a leaf, no matter how small, decomposition at once commences, because the requisite quantity of nitrogen is given off.

433. A box, *a*, *b*, *e*, *e*, of a cubical shape (*fig.* 59), and nearly 12 inches in each of its dimensions, had one of its sides taken out and replaced by a trough, *k k*, of suitable size, consisting of two glass plates cemented at a distance of one fourth of an inch from each other. This trough was filled with a solution of bi-chromate of potassa; one of the sides of the box was hung on hinges, *e e*, as a door, for the sake of obtaining access to the interior. Within this little chamber, a matrass filled with carbonated water, and enclosing a bunch of pine leaves, its neck dipping beneath the surface of some water in a cup, was shut up and exposed to the sun's rays, which, passing through the trough, impinged upon it. In a short time air-bubbles were copiously given off, and when a sufficient quantity was obtained for analysis, its constitution was determined. The following is selected from a number of analyses, being probably the most correct, and very nearly the mean:

Carbonic acid	16·00
Oxygen	8·16
Nitrogen	4·84
	29·00

We here remark the existence of a far larger proportion of carbonic acid, but the relative proportion of the oxygen and nitrogen is still observed with tolerable accuracy; the deviation may be satisfactorily referred to disturbing causes. The greater amount of carbonic acid, as compared with sections (427) and (431), may likewise be due to the higher temperature of the arrangement when shut up in a close box, where currents of air, or other cooling agents, could not have free access to it. Or it may hereafter be found that there are chemical rays of *different colours*, as it were; or, more strictly, of different refrangibility and absorbability, and that those which find a passage through bi-chromate of potassa can cause the decomposition of carbonic acid, though they cannot blacken chloride of silver. The doctrine that chemical rays are nothing more than undulations of an elastic medium, the waves of which vary in breadth, I shall endeavour to support; each of these kind of waves is competent to bring about changes peculiar to itself. Not in this place, however, to anticipate what I have to offer on these matters, I shall continue to use the term *chemical rays* as expressing those which blacken chloride of silver; and these, I say, are not engaged in the decomposition of carbonic acid.

434. From the first observations made on the decomposition of carbonic acid by Priestley, this subject has afforded much scope for chemical speculation. Count Rumford examined it successfully, but wanting means of accurate gaseous analysis, and, above all, not understanding the doctrine and laws of interchange through tissues, his

conclusions are devoid of that degree of precision which the advance of chemistry, in all its departments, enables us to attain. The conclusion to which these earlier philosophers came was, that plants had the power of absorbing carbonic acid from the air, and rendering oxygen in return by elaboration from their vessels; and this they regarded as the great means employed by nature to maintain the integrity of the composition of the atmosphere. A similar view has been taken of this subject by almost every philosopher who has since examined it. Professor BURNET, to accommodate the theory to the observed facts, infers that plants exercise two functions, the one of breathing, the other of digestion, the latter only occurring during the stimulant action of the sunshine. This phenomenon is, however, unquestionably, one depending on the exalted capillary action of a tissue when radiant matter impinges on it; and the evolution of nitrogen, or of some other gaseous or vaporous matter, is, therefore, an essential part of the process.

435. The analyses made in the foregoing sections show that the volume of gas which remains after action is complete, is exactly the same as the volume of carbonic acid first operated on. The best method of proving this directly is to take a tube, the diameter of which may be half an inch or upward, which is graduated into inches and decimal parts. Fill it with water, from which all gaseous matter has been expelled by long-continued boiling; place a few vegetable leaves in it, carefully removing any bubbles of air which may be attached to them; invert the tube in a vessel of water, and pass into it as quickly as possible a measured quantity of pure carbonic acid, and transfer it to a mercurial trough. This arrangement is seen *fig.* 60. Conduct the experiment first in a cool, dark place; absorption will rapidly go on, and in a short time all, or the greater part of the carbonic acid will disappear, a column of mercury, *e e*, rising in the tube to replace the gas. It is to be remarked that it is not always easy to procure the entire absorption of all the gas, a little bubble remaining in the upper part of the tube, containing the impurities that may have existed in the gas, and also any remains of the carbonic acid, for the amount absorbed depends upon several circumstances, as the relative proportion of the volume of gas to the volume of water, the height of the mercurial column suspended in the tube, the temperature of the arrangement, &c. Then, on exposure to the solar rays, gas is copiously given off, the quantity continually decreasing until farther exposure ceases to evolve any more. On making the usual corrections for temperature and pressure, the aggregate of evolved gas will be found precisely the same as the volume first operated on.

436. Sometimes, however, the volume is increased by an amount varying from ·10 downward, due chiefly to a certain amount of gas given off from the leaves extraneously, and partly to the capillary action of the whole system upon the elements of atmospheric air, which are transferred by slow degrees to the water operated upon, should there be a film of that fluid between the mercury and the sides of the glass tube; but, by making allowance for these disturbing actions, the proportion of equality will be found to be rigidly observed by the absorbed and the evolved quantities.

437. We find, therefore, that the evolution and decomposition of carbonic acid by the solar ray are due to that part of it exciting heat; that the chemical ray has no di-

rect agency in the matter; it may bring about changes which, to a certain extent, complicate the phenomenon, but it does not produce the abstraction of any compound of oxygen and carbon from carbonic acid. Apart from the agencies exercised by the elements of the plant, agencies which are unquestionably of the utmost importance, the decomposition is remotely brought about by the action of radiant matter. But non-luminous heat, though capable of evolving gas, produces no change of its constitution; shall we, then, suppose that there is a difference, in point of quality, between the heat given off by the bodies below ignition, and the heat of incandescent matter? Or does the light itself aid decomposition? An experiment may be made which appears to me to bear directly on the answer which should be given to this query. Let a beam of light (*fig.* 61), two inches in diameter, pass through an aperture in the shutter A B, and fall upon any medium, *c d*, which absorbs a certain number of the rays of heat, as bichromate of potassa, which may be so diluted as to absorb exactly 50 rays out of every 100. Having, by means of a good thermometer, *g*, measured this, let the beam of light pass through a second trough, *e f*, containing the same solution of the same strength, and its temperature again be taken, it will appear that the ray, instead of losing half its heat, will contain nearly all of it; or, in other words, the second trough exerts no action on the passing beam. In an experiment tried after the manner here indicated, the thermometer having shown a loss of 50 rays by the action of the first trough, fell only to 47, or gave a loss of three rays only as the action of the second trough; an action to be referred, undoubtedly, to a degree of turbidness which does exist, to a small extent, in the clearest solutions; and also to the reflective and scattering action of the surfaces of the troughs. Now the very same thing takes place in the case of light. A beam that has passed through a green, or any other coloured glass, loses much of its intensity, but if it pass through a second plate, of the same tint, the second loss is entirely disproportionate to the former; and the reason of this is very apparent, for if the second plate had been of a different colour, the ray might have been much more affected, or even entirely extinguished. Delaroche made an identical observation in the case of non-luminous heat, for he proved that a plate of glass obstructed a large portion of the rays falling on it, but that a second plate allowed these rays to pass with far less loss. Now these experiments would lead us to conclude that there are essential differences in radiant heat analogous to the differences in light. The rays of heat given off by a canister of hot water may be, to use an expressive solecism, *violet* heat, and a piece of transparent glass may be able to transmit *green* heat *only;* hence, in using two plates, the absorptive action of the first has the largest share in producing the phenomenon, the second transmitting nearly all which passed the first, an action identical with that of coloured glass on light. Bodies, as their temperature rises, emit more and more rays capable of passing through glass, simply because they become of that class over which the medium does not exercise an absorptive power.

438. The general conclusion which we are to draw from these researches is, that the decomposition of carbonic acid gas by vegetable matter is a very complex phenomenon, due to the combined action of three forces: 1st, the decomposing action of a tissue; 2d, to the impinging of radiant heat; 3d, to chemical affinity, it being probable that any

of these alone would be incompetent to produce this result. And in the case of gas, such as oxygen, being evolved from spring water, we are to refer the change to the ready decomposition of capillary compounds, compounds essentially distinct from chemical, and which can suffer decomposition by the force of capillary action. The colorific and the chemical rays have no influence in this latter case.

439. Non-oxygenation of Phosphorus.—It is stated in the books that Ritter, in making observations on the slow combustion of phosphorus at common temperatures, found that it emitted white fumes in the invisible red ray of the solar spectrum, but in the invisible violet, phosphorus in a state of oxygenation was instantly extinguished. As a similar action is alleged to take place when the sun's rays shine on ignited carbon, it becomes desirable to understand the mode of action: the original experiment of Ritter was therefore repeated, with a view to ascertain its accuracy. A cylinder of phosphorus, *a b* (*fig.* 62), an inch long, and about one sixth of an inch in diameter, was shielded from the action of aerial currents by a glass jar. In front of the jar an equiangular prism of flint glass was placed, so that the rays of a decomposed beam of light coming in through the shutter, *c d*, could successively be thrown on the phosphorus, which was placed horizontally in the jar; the beam of light also came nearly horizontally into the room, reflected by the arrangement already described. Situated thus, by turning the prism on its axis, any ray could be made to cover the phosphorus: the temperature in the shade being 72° Fahr., a fine sheet of metaphosphoric acid, mingled with vapour of phosphorus, so thin as to be almost imperceptible, except in certain positions, was observed to be rising from the cylinder; sometimes it would form a fine cloud, which hung for a moment on the phosphorus, and then rose gracefully in curled wreaths. The extreme mobility of this cloud was remarkable: even the warmth of the observer, by causing currents within the jar, would affect it; if the hand approached, as at A (*fig.* 63), the phosphoric vapour came to the side of the vessel, as it were to meet it, and then rebounded and circulated along the top of the jar. The size, position, and shape of this cloud, when enveloped in the red light of the prism, were deliberately marked; its motions were merely more capricious than when in the shade. And now, by turning the prism, the extreme *violet* ray was brought upon it, but neither did its motion, nor magnitude, nor figure appear in any wise changed.

440. The impression conveyed by Ritter's experiment is, that the chemical rays possess the faculty of hindering oxygenation. The negative conclusion here arrived at might be due to local circumstances, and be referred to the action of the prism, as to its composition, to the state of the atmosphere, &c.; but no better success attended a variety of trials made on different days, and with prisms of crown glass, turpentine, and water. Trial was therefore made of absorbing media, a beam of light being made to pass at one time through a solution of sulphate of copper and ammonia, and at another through bichromate of potassa, but the condition of the phosphorescent cloud was found to be too rough an estimate of the real action. A cylinder of glass (A B, *fig.* 64), ·75 inch in diameter and 3 inches long, was therefore fitted at its upper end with a stopcock, *a;* its lower extremity was closed air-tight with a cork, through which an inverted siphon, *b b*, passed, each of its limbs being four inches long, and the bore

being $\frac{1}{8}$th of an inch; the outer limb was fitted with a scale. After having opened the cock, *a*, a stick of dry phosphorus, *e*, was suspended in the cylinder, which was made very clean and dry, and the siphon being filled with water, was firmly seated in its place and the cock closed. Now, as the phosphorus oxydized, the metaphosphoric acid was removed by the water present, and the level falling in the lateral limb, indicated what quantity of oxygen was consumed, and, therefore, the rate of combustion of the phosphorus. This was expected to give a more accurate estimate of any changes occurring in the phenomenon, and was accordingly applied to detect them.

441. Beams of light of different colours being made to pass at different times through the cylinder, so as to impinge on the phosphorus, attempts were made to ascertain the rate of combustion for each, as also for the white light of the sun, and in the shade. In each insulated experiment, the fluid in the gauge sunk with great regularity, more rapidly at first, and then more slowly, but the same regularity was not observed in different trials. At one time the phosphorus would consume with more than double the rapidity than it did at another, though to all appearance under identical circumstances. If the slow combustion of phosphorus be at all affected by the action of solar light, it is certainly not to that extent which Ritter supposed. So far from extinguishing, the violet rays do not exert any control over it, or if they do, it is to so small an extent that the most delicate arrangements fail to detect it.

442. It is possible, however, that atmospheric temperature may exert an influence on the result. During the trials here made, a thermometer in the shade ranged from 70° Fah. to 82° Fah. At these points the affinity of the combustible material for oxygen may be so exalted that the action of any weaker force becomes masked. It is not stated what were the temperatures at which the alleged results were obtained. But it is most probable that the presence of extraneous matter was the cause of all these variations. It is well known that certain compounds of hydrogen and carbon, in extremely minute quantity, will entirely put a stop to the oxydation of phosphorus; and during the course of these trials, I have had abundant reason to notice errors arising from this cause. By simply wiping out the cylinder with a linen cloth, which contained an almost imperceptible trace of spirits of turpentine, an erroneous result like that of Ritter was at once obtained.

443. Decomposition of the Salts of Silver.—Several of the salts of silver undergo a remarkable change when exposed to the rays of light, the bromide, the chloride, and the nitrate being very good examples; these, which are all white, become of a dark colour, approaching almost to black, when exposed to the violet rays; it is stated that the bromide is most readily affected, yielding a brownish-black colour.

444. If a piece of paper be soaked in a solution of nitrate of silver, and then dipped into a solution of bromide of potassium, it affords a very advantageous means of making these experiments. The chloride may occasionally be substituted for the bromide of silver.

445. A beam of light (*fig.* 65, *a a*) entered a dark chamber horizontally, and was obstructed in its course by a plane metallic screen, *b*, having a hole half an inch in diameter in it. The beam, *c*, which passed through this aperture fell upon a flint

glass equiangular prism, *d*, and was dispersed by it, the spectrum, *e f*, being received on the table; this spectrum was about three inches long. And now a piece of paper, imbued with bromide of silver, was placed to receive it, with the intention of ascertaining how far the discoloration would extend. In the course of five minutes a very marked change had taken place, and, on examination, it was found that the deepest tint had been occasioned where the violet blended with the indigo rays; beyond this, even in the dark space beyond the spectrum, there was a stain, as also as far in the spectrum as where the green light merged into the yellow, an effect represented in *fig.* 66, *a a*, *b b*, being the spectrum; during this experiment the spectrum was kept stationary. Again, a column of light, three inches in diameter, converging from a convex lens (*a a*, *fig.* 67), was intercepted by a screen of pasteboard, *b b*, which had a circular aperture in it half an inch in diameter; this screen was placed at such a distance from the focus, that the circular section of the cone of light was half an inch in diameter, and, therefore, passed exactly through the aperture; a piece of the prepared bromide paper was then fastened on the back of the screen, so as to receive the condensed rays which passed the aperture. In a few moments a black spot appeared about the central parts of the paper, and at the end of the experiment there was an intensely black circle, surrounded by a brown, ring-like penumbra, as in *fig.* 68; the diameter of the black spot being three quarters less than that of the aperture through which the light passed.

446. Interference of the Chemical Rays.—Under certain circumstances, two aerial vibrations, each of which, if separately striking the organs of hearing, would produce a musical sound, may so interfere with each other as to produce an unmelodious rattling, or even silence. Also, two rays of light, whose paths bear a certain relation to one another, instead of increasing each other's intensity, may have a directly opposite effect, and, neutralizing each other, produce darkness. It becomes, therefore, a question, not only of mere curiosity, but one whose bearings are important, to find if the chemical rays emitted from the sun, when placed under similar circumstances, exhibit similar phenomena. For then analogy would lead us to know that it is possible for two rays of heat to be so situated with regard to one another, as, instead of exalting the temperature of the body on which they fell, to lower it; or, in other words, to produce actual *cold*.

447. In my early trials for the solution of this question I met with many disappointments, but at last I fell upon an arrangement which yielded positive information. It is, however, an experiment requiring careful manipulation. A horizontal beam of light being projected into a room by the apparatus heretofore so often referred to, at the extremity, *e e* (*fig.*69), of the brass tube, a double convex lens of short focus was screwed; this brought the rays to a point at a distance of three quarters of an inch from the lens: here they were obstructed by a metallic screen, *b b*, having a round hole, *c*, one eighth of an inch in diameter, perforated in it. This screen revolved about a vertical axis on a pillar, *d*, so that it could be brought to any angle with the incident rays. The rays passing through the round hole, *c*, were received on a white screen, *g g*, at a distance of six inches. When the screen *b b* received the incident rays perpendicularly to its surface.

then, of course, the image thrown on the screen *g g* was circular; but if the screen *b b* was made to receive these rays at an acute angle, then the image was lenticular. Under the last condition, the phenomenon of diffraction is represented in *fig.* 70, where *a a* is the screen, *b b* the lenticular image cast on it; it is of bright white light except at its central part, *c*, where there is a dark image produced by the interference of the passing rays.

448. If, in such an arrangement, the chemical rays do not interfere with each other so as to neutralize effects, chemical action should be produced in every part of the image, even including its central part, *c;* but if, on the other hand, these rays are obedient to the same laws as the rays of light, then, in the central parts of the image, no chemical effects should ensue; the problem is, therefore, reduced to the finding how any compound, changeable by these rays, will comport itself on the central and peripheral parts of such an image.

448. In place of the screen *g g*, a substitute was used consisting of two thin plates of mica, with a layer of bromide of silver included between them; these were mounted in a little ivory frame, *a b c d* (*fig.* 71), just in the manner that objects are usually mounted for the use of the microscope, and the lenticular image cast upon the bromide. After an exposure of five minutes, during which care was taken to keep the sun's place perfectly immovable, and also to avoid all local tremour, which might make the image traverse on the bromide, the result was very apparent, being, as represented in *fig.* 72, of the natural size; the peripheral parts being of a deep brown, and the centre yellowish white. Viewed through a lens, the boundary line was not sharp and distinct, but seemed to merge by an insensible gradation into the unaffected part, as in *fig.* 73. The conclusion to be drawn from this result possesses no common interest; for the same reasoning which demonstrates that light consists of undulations of an elastic medium, applies here also.

450. The chemical rays, thus closely attending the luminous rays, and being, like them, subject to the forces bringing about reflection, refraction, and interference, it would become a matter worthy of inquiry to find whether there be any different classes of these rays analogous to the different coloured rays of light, or the unequally refrangible and absorbable rays of heat. The salts of silver are only one of a class over which the chemical rays exert an action. The following list contains, I believe, all the metallic salts at present known, in the constitution of which changes are brought about by exposure to the sun:

Chloride of manganese,	Iodide of mercury,
Sulpho-cyanate of iron,	Chloride of mercury,
Sulphate of nickel,	Bichloride of mercury,
Carbonate of lead,	Chloride of silver,
Carbonate of nickel,	Bromide of silver,
Nitrate of bismuth,	Sulpho-cyanate of silver,
Chloride of uranium,	Nitrate of silver,
Sulphate of uranium,	Bromate of silver,
Nitrate of uranium,	Chloride of gold,
Chloride of copper,	Chloride of osmium and potassium.

Besides which, there are two others, whose constitution is not well known; one prepared from an alcoholic solution of the double chloride of platinum and sodium, by the action of chloride of potassium, and the other in a similar manner from the cyanide of platinum.

451. The changes which these bodies experience are of different kinds: some become black and some turn white; some, as the sulphate of nickel, undergo change of crystalline arrangement. If we are to take the chloride of silver as a type of those bodies which undergo partial reduction, it will be found probable that the change impressed on them is only superficial, as analysis will show. But we cannot tell with certainty whether a perfect reduction of some of these compounds takes place, or whether it is a sub-salt of a dark-gray colour that results. By taking advantage of the property which chloride of silver possesses of subsiding very slowly from neutral solutions, so as to make them assume a milky consistency, we may present it in a state extremely favourable to the action of the solar ray. For if a thick mass alone be exposed, the central parts will not undergo the same change as the exterior, being shielded by them from the sun. A milky solution will, after an exposure for a certain time, become quite clear, the chloride precipitating, owing to the liquid becoming acidulous. Mechanical agitation being then resorted to, to expose fresh surfaces of the precipitate very frequently during a period of eight or ten days, and care being taken to suffer no dust or other impurity to enter the vessel, it will be found that the powder has become of a reddish gray, interspersed with little particles of unchanged white chloride; these, from their superior density, will have precipitated more readily than the gray particles; washing and decantation will therefore readily effect a separation of them. One hundred grains of the dark chloride thus treated yielded, on analysis, 79·3 of metallic silver; that quantity contains, therefore, 20·7 of chlorine; it has lost by exposure 5·3 grains of chlorine of the quantity originally contained in it.

452. Other analyses, of the same sample, furnished results not widely varying from this, but such is not the case with analyses of different samples; these give sometimes more, sometimes less chlorine; they prove that the chloride of silver, as darkened by light, is not a definite compound, but rather a mechanical mixture; that the change of composition is chiefly confined to the surface, and does not affect the interior of the particles to any extent; it is true that microscopic observation shows them to have a uniform consistency and colour, but of course reveals nothing of their internal character. An error is frequently made by writers who describe the changes happening in this partial reduction; it is not, as they say, hydro-chloric acid which is evolved when the chloride is under water, but it is chlorine, as is made very evident by the strong, disagreeable odour of that gas when the experiment is conducted in close vessels.

453. In addition to the list given above of substances changed by the chemical rays, there are some others which exhibit their action in a very marked manner. Chlorine and hydrogen unite together with an explosion; carbon and chlorine are also made thus to unite in producing the per-chloride of carbon; all kinds of vegetable colours are bleached; hydriodide of carbon and chloro-carbonic acid are always made by the action of solar radiant matter.

454. It has been stated by some chemists that, while the violet extremity of the solar spectrum blackened chloride of silver, there are other parts of it which would bleach the salt so blackened. But it is not so, for neither does any part of a very dispersed spectrum, nor a beam which has passed through a variety of absorbing media, exert such an action. These experiments I tried repeatedly, under all the conditions of variation of temperature and brilliancy of the solar rays, but no observation led to the inference that there was any change of colour, or any sign of an approaching change, even after the lapse of a whole month. Indeed, it would seem that the state of the case does not justify any such expectation; when the chemical rays have disengaged the chlorine, it is gone, and lost forever to the silver, being scattered abroad in the atmosphere; if, therefore, the substance ever regains a white colour, chlorine must have been purposely furnished from other sources, or the white substance said to result must be some compound of unknown ingredients.

455. The light of the moon is a remarkable example of luminous rays existing without either calorific or chemical rays; the most delicate thermometric arrangements have hitherto failed to show any rise of temperature in the moonshine. A piece of paper, imbued with chloride of silver, may also be exposed to the rays of the full moon, converging from a glass, and it will not exhibit any change; this I proved by placing such a paper in a situation where, for a whole night, the rays of the moon could reach it. And the same observation applies to terrestrial flames. In none of these has the existence of the chemical rays been detected. Chloride of silver, after being exposed for eight hours to the bright flame of an argand lamp converged by a lens, retained its whiteness. The same effect was witnessed when the flame of alcohol tinged red by strontian was employed, or the yellow flame produced by chloride of sodium, and the green of boracic acid; in these cases the periods of exposure did not exceed half an hour.

456. Of Perihelion Motions.—Probably the most remarkable effect exhibited by the solar rays is the motion they produce in media endued with much mobility. For many years it has been known that camphor exposed in a bottle to the rays of the sun, formed a crystallization on that side of the vessel nearest the luminary; but the action is so slow, and requires such a length of time for its completion, that no successful investigation has been made as to the nature of the forces in operation. Some philosophers have assumed, upon insufficient ground, however, that the crystallization was effected on the most illuminated side, merely because it was the coldest, as we know that vapours are always deposited on that part of a surface the temperature of which is the lowest.

457. About three years ago, I published a series of observations on this point. Having found, from some theoretical considerations, that the crystallization of camphor took place in vacuo with a rapidity convenient for experimental investigation, I was led to make an extended inquiry into the whole matter.

458. The sun's rays have the power of causing vapours to pass to the perihelion side of vessels in which they are confined, but, as it would appear, not at all seasons of the year. For example, I have a certain glass fitted up for making these observations, and in this vessel, during the months of December, January, and part of February, 1836–37, a deposite was uniformly made towards the sun; during the months of March, April,

and part of May next following, although every part of the arrangement remained to all appearance the same, yet the camphor was deposited on the side farthest from the sun. From May until the present date, the deposite is again towards the sun. It does not appear that any immediate cause can be assigned for this waywardness. Does it exist in the sun's light? or in changes affecting the earth's atmosphere? or in imperceptible changes in the instrument with which the observation is made? As respects the latter, I think a negative answer may be given without any hesitation; but beyond a mere expression of the fact that these anomalous circumstances do occasionally occur, I would not be understood to speak decisively; if periodic changes like this do occur, which is doubtful, they have not been watched for a sufficient length of time, nor have I made sufficient variations in my trials to be able to refer them to any distinct cause. A large bottle containing camphor, which has been deposited therein for more than a year under ordinary atmospheric pressures, has uniformly showed a crystallization towards the light.

459. For making these experiments properly, it is necessary to possess an air-pump receiver ground so true as to be able to maintain a vacuum for several hours, or even days. A less perfect jar may be made to answer by fastening it down to the pump plate with cement; it will, however, be liable to leak when the cement becomes warm by exposure to the sun. For many of these trials a barometer tube is sufficient. Those who are provided with a good pump and jars, accompanied with their proper transfer plates, will have no difficulty whatever.

460. Upon the plate of the pump, or one of the transferers, *a a* (*fig.* 74), place some camphor in a watch-glass, *c*, supported by a stand; over this place a bell-jar, and exhaust until the difference of level of the siphon gauge amounts to half an inch or less—the farther the rarefaction is pushed the better—then remove the arrangement into the sunshine. In the course of five minutes, if the atmosphere be clear and the sun bright, small crystalline specks will be found on the side nearest to the sun; these continually increase in size, and at the end of two hours many beautiful stellated crystals, from one eighth to half an inch in diameter, will be found on that side, but on the other parts of the glass only a few straggling ones here and there. This appearance is represented in *fig.* 75; sometimes the whole side next the sun is covered with a deposite of camphor, the other side containing none at all.

461. Having made a torricellian vacuum, in a tube upward of 33 inches long and five eighths wide, pass into it a piece of camphor, which will rise into the void. This arrangement, like the former, when kept in the dark shows no crystallization, even though so kept for more than four months; but on bringing the vacuum into a beam of the sun, crystallization rapidly goes on, and at the end of a quarter of an hour the appearance is such as is represented in *fig.* 76. It is not important that the temperature of the sunbeam or of the atmosphere should be high; this is an experiment which will succeed at temperatures varying from 120° Fah. to 60° Fah., and probably at much lower degrees, for it is readily performed in the depth of winter.

462. It is not a phenomenon connected with the process of crystallization. Take a jar twelve inches high and four in diameter, quite clean and dry, place it over a glass of water, *b* (*fig.* 77), and expose it to the sunshine. In this experiment it is not re-

quired that there should be a vacuum within the jar. In the course of an hour or two, there will be a copious dew at *a*, and on farther exposure drops of water will trickle down the side of the glass, but on the opposite side not the least cloudiness will be found.

463. Barometers, hung up in such a position that the sun's rays can have access to them, exhibit an analogous appearance, the side nearest the light being studded with metallic globules.

464. In any of these experiments iodine may be substituted for camphor, provided mercury is not present, nor any other substance on which this body acts; the most advantageous method of using iodine is by heating it in a suitable vessel, and when the vessel is quite full of vapour, presenting it to the sun's ray; deposition goes on, on the sunny side, as the condensation takes place.

465. Nor is it requisite, in obtaining these results, that the material should be either gaseous or vaporous. The rays of light have the property, as was found by Count Rumford, of decomposing an aqueous solution of chloride of gold; on making this experiment in a test tube one third of an inch in diameter (*fig.* 78), small spangles of metallic gold will be seen, by reflected light, on the side towards the sun, *b;* by transmitted light it appears of a pale green tint, as is the colour of gold leaf. Here we find that, under certain circumstances, solutions will deposite metallic matter, in obedience to the same laws which cause the crystallization of camphor and the deposite of aqueous dew.

466. A few pieces of camphor were laid on the plate of an air-pump, and a circle of glass two inches in diameter (*a, fig.* 79) was supported on a pedestal in the midst of them, the upper part of the glass being four or five inches above the pump plate; it was then covered with a jar, and exhaustion performed. On exposure to the sun for a suitable length of time, numerous crystals were found on the jar, but none on the circular plate, although it had received the full beams of that luminary. This experiment was made with a view of determining what peculiar condition a glass surface was placed in by exposure to the light; for experimental purposes, the rounded form of the glass receivers being very unsuitable. It was not, therefore, without surprise I observed that, however long the plate was continued in the beams of light, no crystallization would ensue. A flat surface, however, being essential in the course of experiment pursued, trials were repeatedly made, by various changes in the arrangement, to cause a deposition of camphor upon such a crown glass plate; but though in five days I could procure starry crystals upon the bell jar of more than half an inch in diameter, in no instance was a solitary one found on the glass plate.

467. Two circumstances may determine the precipitation of camphor crystals on a surface: 1st. Reduction of temperature; 2d. Increase of pressure. To the former we cannot look for an explanation in the case before us, for there is an actual increase of temperature in every part, and more especially on that side of the vessel which is next to the sun. Why, then, does this condensation take place on the hottest surface, the side nearest to the sun? we cannot admit that the rays of heat have any active part in bringing about the phenomenon. On the other hand, they ought rather to exert a contrary effect, antagonizing the powers that solicit the camphor crystals to form,

and driving them to the coldest surface. We are therefore reduced to the supposition, that when the light of the sun impinges on a surface of glass, it places that surface in such a condition that it exerts a pressure on the adjacent medium, immediately followed by a condensation of that medium. The state of the force here spoken of applies to the glass surface alone; it is not an action between the solar ray and the forces that produce crystallization, seeing that it equally takes place in the deposite of aqueous or mercurial dew, and even of solid gold from a solution of its chloride. In other words, if a ray of the sun be incident on a surface of glass, it develops a force of attraction on that surface.

468. A gaseous medium, having its temperature disturbed at any point, has a current determined in it. In a chamber, such as the bell of an air-pump, this current circulates round the walls, ascending on the hot and descending on the cool side; it might be supposed that to this circumstance was due the fact of no crystals being found on the plate of glass (466). The condensation cannot, however, be attributed to this cause; for if so, a lamp, or any other source of heat, would be equally effectual; it will, however, be hereafter shown that artificial flames tend to remove these depositions from the side nearest to them, and cause them to be accumulated in the colder regions.

469. Beneath a receiver (*a, fig.* 80) a cubical bottle, *b*, having flat sides, was placed, and in the bottle a few pieces of camphor; the mouth of the bottle was about half an inch in diameter, and was left open, the pressure of the atmosphere being reduced to 1½ inches of mercury. Temperature of the ray 57° Fah. On examination, after the lapse of one hour and twenty-five minutes, no crystals whatever could be found on the receiver, and but a few sparsely scattered on the sides of the cubical vial. Now there can be no doubt that the whole receiver was full of camphor vapour, and it does not appear that any reason can be assigned for the anomaly of its non-crystallization.

470. Will artificial light produce analogous results? To ascertain this, I took a glass globe about one inch and a half in diameter, with a neck four inches long, fitted it with a stopcock, and introduced within it a drop of water. The vapour of this water exhibited extreme mobility; exposure to the clouds caused its immediate deposition. A farther advantage was gained by the use of this apparatus, for by heating the globe uniformly, until all the moisture on its surface was vaporized, and then allowing it to cool, the particles of water readily obey the forces that solicit them. This glass globe, supported vertically on an appropriate stand (*a, fig.* 81), was placed at a distance of nine or ten inches from a brightly-burning argand lamp, A; to protect it from accidental currents of air, and from irregularities of radiation from other sources, the whole arrangement was covered by a bell, *b c*, open at both ends, and about fifteen inches high. It appeared, at first, that a thin dew lined the inside of the whole globe, instead of being confined to one part; but after a certain space of time, the heat which passed from the lamp through the protecting glass disturbed the results, the dew being driven to the coldest parts. To get rid of the effects of this heat, at a distance of about three feet from the lamp (A, *fig.* 82), a double convex glass lens, *c*, 2½ inches in diameter, was placed, which brought the rays to a focus at a distance of five or six feet, where stood the glass globe, *a*, covered with its protecting jar. The globe had been previously slightly warmed,

so as to expel all the dew from its surface, and give it a uniform temperature; in several trials it was found that there were no evidences that the bright flame of an argand lamp exerted any force soliciting the vapour of water to move towards one part of the glass rather than another.

471. I took the arrangement of 462, and shut it up in a dark closet, having previously made the jar perfectly clean and dry; it remained there for several days, that it might be found whether those little irregularities of temperature which occur in such confined chambers would cause this dew to pass to one side of the glass rather than another; it did not appear that such was the case, for the glass was as free from moisture when taken out as when shut up. And now, this arrangement being placed in the window, where the sun was brightly shining, exhibited on its perihelion surface, in the course of three and a half minutes, a pearly dew; and in six minutes drops of water were trickling down that side.

472. But it is not essential to the success of this last experiment that the solar ray itself should impinge on the vessel. The temperature in the shade being 94° Fah., I placed the receiver with its cup of water in a window having a northern exposure, and found that the dew readily made its appearance on that side which was towards the light.

473. It might be suggested that when a vessel is exposed to the sun, that part of the glass which is nearest to him may actually be the *coldest;* such an opinion, it is evident, rests on no sufficient grounds. A jar, *a g* (*fig.* 83), was taken, of such dimensions that it could receive the differential thermometer, *c d b*, the balls of which, *b* and *c*, touched the opposite sides, and in the dark the liquid stood at zero, but on bringing it into the sunshine, if the side *a* was exposed, then the ball *c* was warmest, and if the side *g*, then the ball *b* was warmest, as was indicated by the motion of the liquid. Hence we know, that in all cases where crystals of camphor, dew of water, &c., are deposited on the side next the sun, they are so deposited in opposition to an energetic force which tends to remove them.

474. Light which has suffered reflexion at certain angles seems to have undergone a remarkable modification, being no longer able to put the glass into such a condition that it can cause motion towards the sun. It is not to be inferred that any connexion is here traced between this disturbance of the condition of light and the change impressed on it by polarization. A beam of the sun falling on a plate of glass, and being reflected at an angle of 45°, may be intercepted by any of the arrangements of sections 460, 461, as by the barometer tube. It will be found that the crystallization proceeds with considerable rapidity, not, however, on the perihelion side of the vessel, but on the opposite side. It is probable that this result is not dependant on the polarization of light, inasmuch as it takes place equally well at all the angles, less and greater than the maximum angle of polarization of glass. Light, even that of the sun, having once undergone reflexion, has received some determinate impress, which disables it entirely from causing camphor to crystallize on the perihelion side of vessels.

475. Another remarkable phenomenon is exhibited by the following arrangement: Take a receiver, *a* (*fig.* 84), twelve or fifteen inches high, and three or four in diameter;

place it, as usual, upon the transfer plate, with its proper charge of camphor, *c*. Then cover it with a tin cylinder, *e f*, of sufficient dimensions, so that all the light may be shut out except at one place, *g*, where there is a hole half or three quarters of an inch in diameter. Under favourable circumstances, as a serene sky and bright sun, let the arrangement be exposed, that a column of light may pass through the aperture *g* into the glass; it may or it may not finally fall on the camphor at *c*. It would, of course, be expected that a collection of crystals would form on the inner surface of the glass corresponding to the aperture *g*. But on trial it is not so; for however bright the sun may shine, or however favourable other circumstances may be, not a solitary crystal will make its appearance either there or on any other part of the vessel, provided its temperature has been pretty uniform. On an exceedingly calm and serene day in July, 1835, when every circumstance seemed propitious, I made this experiment, and because the jar that I was using was not ground sufficiently true to fit the transfer plate accurately, it had been fixed thereon with common cap cement, and on exposure to the sun, the temperature of the whole arrangement rose so high that the cement was in almost a semifluid condition; it was one of those days when the eye cannot behold the sky or look on the ground without pain, yet not one crystal could be made to appear opposite to the aperture. But on taking off the metallic screen, and exposing the jar, in a little more than a minute small specks were observable on the glass, and in a quarter of an hour its perihelion side was densely coated with crystals. How are we to explain this? Do the edges of the aperture *g* impress any change on the passing light? Or is the glass surface placed in such a condition that it can no longer produce the deposite of crystals? We shall see hereafter that there are circumstances yet more remarkable, which put us in possession of an explanation.

476. For the proper understanding of the rationale of these experiments, it is required to know whether it be essential that the solar ray should impinge on the camphor or not, or whether the action is exerted on the vapour only. A tube was therefore taken, of suitable dimensions; in the lower part of it a fragment of camphor was deposited, and screened as much as possible from the rays of the sun, while its upper part was freely exposed. Crystals formed without difficulty at a distance of three or four inches, or even a foot, from the camphor, but there appeared to be a limit beyond which they did not readily pass. A tube four feet six inches long and two inches in diameter being exhausted, did not show on its exposed end any appearance of crystallization. Near the camphor the deposite was pretty copious, but in advancing from it the crystals were more sparsely scattered, until, towards the upper extremity, none could be seen. Now the maximum quantity of vapour that can exist in a void, or among other gases, provided the mixture be in equilibrio, depends on the lowness of the temperature of any one part of the vessel; and hence, a long tube, one of the extremities of which is kept cold, does not exhibit these configurations readily, because the quantity of vapour in it is small, owing to the coldness of one part of the void space. It is not necessary, therefore, that the sun should shine *on* the camphor, the effect of the rays taking place entirely on the vapour filling the void.

477. There is a singular action which certain bodies exert over this process. Take

a receiver, able to maintain a vacuum for some time, and having cut out a ring, *a* (*fig.* 85), of tin foil, an inch and a half in internal diameter, and half an inch wide, paste it upon the receiver, as at *a* (*fig.* 86); moreover, accommodate the receiver with its camphor, as usual, and having exhausted, expose it to the direct rays of light, so that the ring *a* shall be on the perihelion side. In the course of a short time that surface will be found studded in various directions with crystals, as is to be expected; but it will be found that none of these crystals come within a certain distance of the ring, and that not one is to be seen within the circle circumscribed by it. The ring, therefore, exerts a kind of protecting action on the glass, hindering the deposition of crystals within certain limits; such a result is depicted in *fig.* 87.

478. This action of a ring, formed of good conducting materials, might be supposed to arise either from its adding something to the surface of the glass, or taking something away from the glass with which it is in contact. Or it might be imputed to some change impressed on the ray of light. Take, therefore, a ring, *a* (*fig.* 88), and place it *before* the receiver, *b*, at a distance of half an inch, the ring being of the same dimensions as in the last experiment; it will be discovered that, although the ring does not *touch* the glass, it still protects it, no crystals coming within a certain distance of the regions overshadowed by the metal; and even at a distance from the line of shadow not a crystal is to be seen, nor any visible in the illuminated centre.

479. Even after crystals have been formed on the surface of the jar, if it be placed in the sunshine with a ring before it, as in the foregoing experiments, the ring will be found not only to exert a protection on the glass, hindering any farther deposite, but will even *remove the crystals that are there.*

480. This is, indeed, a remarkable circumstance; a part of the perihelion surface is shaded from the sun, and thereby rendered cooler, yet the crystals deposite themselves on the hottest surface, and avoid that where it is cold. We see, now, how it happens that, in the experiment of admitting a column of light through a hole in a screen, no crystalline deposite was effected; the protecting agency of the metal, whatever its power might be due to, hindered it.

481. To give the particulars of one of these experiments. On the 11th of July I prepared an arrangement such as the foregoing: the thermometer in the shade was at 76° Fah., and in the sun at 99° Fah., distance of the ring from the jar half an inch, its internal diameter three quarters, and width half an inch. After proper exposure the jar was examined; there were no crystals on that part opposite the central opening of the ring, and the nearest crystal to the internal border was six tenths of an inch distant from where the shadow was projected on the glass.

482. Vapour of water exhibits similar phenomena; a thin piece of tin foil in the form of a cross, a ring, or any other shape, effectually prevents the deposite of water near it.

483. Instead of placing the ring outside of the glass, now let it be placed on the inside, as at *a* (*fig.* 89), so that it may be within one eighth of an inch of the surface. When the crystals have fully formed, it will be discovered that the ring has exerted the same kind of protecting agency that it did when on the outside of the glass.

484. Hitherto, a class of bodies has been tried as protectors which are without ex-

ception good conductors of electricity, such as the metals. Certain indications led me to make trial of resinous matters, which are non-conductors of electricity. Having made the region about *a* (*fig.* 86) of the air-pump jar very warm, over a spirit lamp, a ring of rosin was spread on it, about the same size as the ring of tin foil which had been formerly there. This ring of rosin was transparent, admitting the light to pass it readily, and at a certain distance appeared of an amber colour. Having arranged the jar as usual, and exposed it to the sun, after a certain length of time well-marked crystals were deposited on the perihelion side, on which the rosin was; these crystals not only came up to the verge of the rosin, and filled also the inner circle, but were found on the rosin itself.

485. Metallic plates of various shapes, and under various circumstances, were exposed with a view of causing condensation upon them; it was not found possible, however, either to cause the formation of aqueous dew or crystalline deposite, except when their temperature was below that of the medium in which they were exposed.

486. At this stage of the inquiry it becomes important to know whether, along with the rays of light, of heat, and of chemical action, there are not also rays of radiant electricity emitted by the sun. Almost all operations which disturb the equilibrium of light and heat, disturb too that of electricity, and it is well known that upon this fact Dr. Hare founds the explanation of the action of certain voltaic arrangements, especially the calorimotor; an explanation the correctness of which later researches make more probable. If light, heat, and electricity are set in motion by the force of chemical action, and are often found coexisting, there is nothing improbable in meeting them together in the case before us. It is very true that, as yet, we have not met with any example of electricity under what we understand as a radiant form, but that it consists of undulations of an elastic medium, like the undulations of light and heat, is not to be doubted. The experiments of Nobili give proof of an interference, analogous to the interference of the rays of light, which has served so well to refer the motions of that fluid to the undulations of an elastic medium; the analogies of light and heat are everywhere kept up, and we look with confidence that they will be extended hereafter to electricity.

487. The tendency of the experiments here communicated is to show that certain substances, conductors of electricity, have the faculty of depriving glass of that power by which it causes the condensation of vapours upon it when exposed to the sun; that deposition will not take place on metallic surfaces, but that certain vitreous and resinous bodies interfere in no manner with the process. The inference appears obvious, that electricity, brought into play in some unusual manner, is the cause of the phenomenon.

488. By the action of the solar ray electricity of high tension can be developed. A copper electrical condenser was taken, the plates of which were about one fortieth of an inch apart, and six inches in diameter; there was nothing more in their construction than is met with in the usual arrangement. Another condenser was also provided, which was connected with a gold-leaf electrometer, the plates being one inch in diameter, and separated from each other by a very thin coat of gum-lac varnish. Trials were repeatedly made to discover whether the apparatus was trustworthy. It is a com-

mon complaint against instruments intended to indicate low charges of electricity, that they furnish evidence of an accumulation when none has been communicated; it is necessary, therefore, to examine an instrument to be certain that this objection cannot be preferred against it. Having obtained this preliminary evidence in a satisfactory manner, and having decided the effectual goodness of the instruments in other particulars, the following trial was made. The six-inch condenser was exposed to the sunbeam for one hour, on a clear, bright day; the charged plate was then parted and applied to the one-inch condenser; the plates of this being parted, a small but perfectly distinct electric action was obtained. This experiment is not, however, devoid of sources of error, as from the friction occasioned by touching the plate of one condenser with the plate of the other, or the heating action of the ray, which might cause currents of air to brush over it; but it was found, by purposely rubbing one plate of the condenser on the other, that no charge of electricity could be produced, even if the friction were continued during some time; and on maintaining the temperature of the condenser at the same point to which it was brought by the sunbeam, in order to produce like currents of air, no divergence whatever of the gold leaves was produced.

489. When the tension of electricity is high, one of the most delicate methods of detecting its presence is by the light it emits in vacuo; the excitation caused by the tremulous motion of a column of mercury in a barometer tube is rendered visible by the bright light it gives out, when no other method could discover it. On this principle, attempts were made to detect electrical action in the sunbeam, by exposing metallic plates of large dimensions to the ray, and causing any electricity they might gather to give out light in the vacuum; these trials did not prove satisfactory.

490. It has been stated (439), that the cloud which rises from phosphorus, when slowly oxidating, is endowed with great mobility; for certain purposes it makes a very good electroscope. When a piece of phosphorus is shielded from the air by a bell jar, and not exposed to disturbing action of any kind, a fine sheet of vapour rises vertically upward. If, at a distance of several feet, an excited stick of wax be presented, the vapour curls from its path, and leans over to the side of the glass adjacent to the cause of the disturbance. If such a jar be exposed to the sun, a like disturbance is exhibited: as soon as the rays fall on it, it seems as though they caused each particle to repel its fellows; the straight column, which before passed to the top of the jar, separates into confused masses, which pass forward to the perihelion side.

491. No direct proof existing that rays of electricity are emitted by the sun, and as it does not fall within my purpose to discuss their hypothetical action, it may be sufficient to give the proof, that if the surface be admitted to be electrified, these deposites should take place. If a receiver be taken, and on any part of its interior surface a glass rod be made to pass, the line which it describes will be stellated with camphor crystals, on exposure to the sun after exhaustion. This curious fact was first observed in the case of an exhausted vessel, which had a small siphon gauge shut up in it, the extremity of which rested against the glass; by accident the gauge was moved half round the glass, and in a short time after a line of crystals was observed coinciding with the line of motion; it was found possible afterward to repeat this result at pleasure; the appearances were such as are represented in *fig.* 90.

492. Upon the hypothesis here assumed, the deposite of crystals becomes a phenomenon analogous to the curious configurations described by LICHTENBURG, when powders are dusted on the surface of an electrified plate; so close is the resemblance, that one who sees crystallization produced by the sun for the first time, would be led, almost involuntarily, to refer both to the same cause; suppose it granted, that when light falls on any surface, that surface is electrified, it will exert an attraction on any particle in its vicinity; but if a conducting substance be placed in contact with the surface, not only will it hinder deposite on the place which it occupies, but also it will rob the glass around it for some distance; here we find an explanation of the action of a tin foil ring. Again, if that conducting substance be so placed as to cast its shadow on the glass, no deposite should take place on that shadow, nor for a certain distance around it, because the electricity of the adjacent parts would pass towards the unelectrified spaces, thus conferring, by a surface conduction, a low charge to all the shaded parts.

493. We meet, however, if we pass beyond these simple explanations, with so many difficulties, that we are not encouraged to seek farther confirmation of this hypothesis; there are some facts which prove, almost demonstratively, that electricity is not the agent in question. If, instead of a ring of rosin, we make use of a ring of sealing-wax or a ring of pitch, these, though they are non-conductors, do not fail to protect; the action of a metallic ring, when placed inside of a jar, cannot, so far as I know, receive any explanation, especially if we are to admit the non-conducting power of a space filled with camphor vapour only. It is plain and obvious that transparency and opacity have nothing to do with it; glass and rosin, it is true, do not protect; but oil, which is equally transparent, protects as powerfully as a metal.

494. Are we to refer this singular action to the rays of light, to the rays of heat, or to the chemical rays? By the action of absorbent media, attempts have been made to determine this question. A barometer tube (*f d e*, *fig.* 91) had a conical tube fixed on its outside, so that the interstice could contain liquids at *c d*, without leaking. Into this torricellian vacuum I passed a piece of camphor, and exposed the arrangement to the sun; having filled the interstice with water, it was found to have crystals on the aphelion side, there being a ring of them, as at *e e*, *fig.* 92, all round the tube. The water was then poured out, and a solution of sulphate of copper and ammonia introduced. On examination, it was found that on the side nearest the sun no crystals were to be seen, but on the other side there was a dense layer of them, extending exactly half way round the tube, and very much resembling the shape of *fig.* 93. A yellow liquid, the bichromate of potassa, was next introduced: a result to all appearance exactly like the former was again produced; but having observed that the thickness of the media had a very sensible effect, apparently due to their becoming warm, and not casting off their caloric with sufficient rapidity by radiation, I made an alteration in the arrangement, by interposing between the torricellian vacuum and the light a trough capable of containing the different solutions. This trough being filled with solution of bichromate of potassa, and the ray tested that it could not blacken chloride of silver, in about one hour the tube presented the following appearance: there were some pretty large crystals which extended round the tube, as at *a* *fig.* 94, which, on the aphelion side, suddenly mounted

up, froming a kind of hyperbola; on the anterior semi-circumference not a solitary one was to be seen. The trough being now filled with sulphate of copper and ammonia, the arrangement of the crystals was found to be in every respect like the former.

495. Supposing that this result might in some measure depend on the ray having been subjected to reflexion before passing through the trough, I repeated the trials when the sun's altitude was small enough to permit the rays to pass without requiring reflexion, yet still the same results were uniformly obtained; so that, whether the chemical or the calorific rays were stopped, crystallization took place on the aphelion side of the tube.

496. May it not, therefore, be that this attractive force originates whenever the calorific ray impinges on a surface? It does not necessarily follow from the phenomena that any peculiar class of rays is emitted by the sun, which bring about this action; but if there are such, it is a question of interest to find what is the reason that good conductors of electricity render their action nugatory.

497. Botanical authors have long been aware of the important effects which solar radiations exercise over the colour of vegetables. A plant which grows in the dark is of a pale whitish colour, and of a transparent aspect, possessing none of that greenness and vigour which are so characteristically developed on exposure to the sun; its consistency is watery, and although its growth may not be stunted, its appearance is very sickly, its secretory actions are not duly performed, and all its vital operations are carried on in a depressed way. There is no longer any evolution of nitrogen from the leaves, and, consequently, no apparent production of oxygen gas. Light, which seems to act merely as a stimulus on the green organs of vegetables, indirectly bringing about the decomposition of carbonic acid, though accessory, is not, however, essential to growth. In subterranean cavities, and places far removed from the direct solar ray, plants have a colour of their own; and in the abysses of the ocean, at depths to which no solar beam can penetrate, and where there is a perpetual night, they are found flourishing.

498. The green colour of leaves is presumed to be an immediate consequence of the act of decomposing carbonic acid. It appears to me that there is some obscurity, if not an actual error, in the view which botanists take of this matter. They suppose that, by the stimulus of light, some portion of the green matter is enabled to decompose that gas *completely*, or to accomplish its actual resolution into an equivalent volume of oxygen, with the entire deposition of the carbon in the solid form; that it is, moreover, this carbon, so deposited, that gives origin to the green colour, seeing it forms the *chromule verte* itself. Much useless ingenuity has been thrown away by some chemists in explaining how carbon, the colour of which is black or a deep blue, can produce a lively green; and even if their supposition that the modifying action of a yellow tissue spread over it were correct, of which there is much doubt, considering the thinness of that tissue and the lightness of its tint, yet certainly we have no necessity to resort to any such explanation. The deposite is not carbon chemically; it contains both oxygen and hydrogen in unknown proportions. Of all the physical characteristics of a body, colour is the most uncertain: after uniting in a new mode, compounds never bear the colours of their constituents; nay, more, carbon itself is not essentially of a black colour, as the diamond proves.

499. To a deposite of some compound, in which carbon enters as an ingredient, we are to refer the green colour of leaves, but not to carbon itself. The earlier chemists, who did not possess those extremely delicate methods of gas analysis which are now available, misunderstood this matter. They stated that, on exposing a plant to the sunshine, in contact with carbonic acid, the carbon was separated in a concrete state, the oxygen being left, but such is not the fact; by no known laws can such a change be brought about, and hence any reasoning based upon it, as to the colour of plants, is irrelevant. For when a plant exposed to the sun decomposes carbonic acid, a certain volume of oxygen disappears at the same time; in lieu of this, and in obedience to the laws which guide the transit of gases through tissues, an equivalent volume of nitrogen is surrendered by the plant in return. Sometimes it is carbonic oxide which is absorbed, sometimes oxalic acid, or other compound of carbon with less proportion of oxygen. I do not here indicate from whence that nitrogen is derived, since botanists assert that some plants contain no nitrogen at all; it may, however, exist in their juices as gas exists in spring water, or may be retained in a compressed state on their surfaces; it is, however, a remarkable fact that nitrogen is always present.

500. The carbon thus taken from the acid does not pass through the tissue of the leaf in a concrete form, or give rise to a concrete deposite; it bears with it a certain part of the oxygen with which it was formerly united, the rest being set free; the carbon and oxygen so conveyed into the plant, entering into combination with hydrogen, give rise to the chromule verte; hence we see that the green colour depends indirectly on the decomposing action; that when this goes on without interruption, that is fully developed.

501. I took five pea plants out of the garden, as nearly resembling each other in size and other particulars as might be: they had just appeared above the surface of the earth, and were beginning to put out leaves. These plants I designate by the numbers 1, 2, 3, 4, 5. Each one was planted in a small glass vessel with a hole in the bottom, for the purpose of supplying it with water, after the manner of a common flower-pot. Number 1 was placed in a box into which light passed which had traversed a solution of sulphate of copper and ammonia. No. 2, in a similar box, into which light was admitted after having undergone the action of chromate of potassa. No. 3 was placed in the open air. No. 4, in a box into which light had passed which had been transmitted through sulphocyanate of iron. No. 5 was shut up in a dark closet. This arrangement was completed on the second day of May. With a pair of compasses the height of each plant was ascertained, and of that and of the number of leaves a memorandum was taken. In three days' time an examination was made.

No. 1 had attained three times its former height, and doubled its number of leaves.

No. 2, not quite twice its former height, no new leaves, in appearance not so plump and transparent as formerly.

No. 3, twice its former size, with no fresh leaves.

No. 4, four and a half times its former size, and double its number of leaves.

No. 5, three and a half times its former size; the leaves looked yellowish.

502. It is here proper to remark, that the increase of size is not to be taken as an

index of any action of the absorbing medium. Some years ago, I had occasion to notice that rapidity of growth was greatly influenced by the quantity of aqueous gas in the atmosphere. Whether the observation possesses any novelty, I am not prepared to say; but if any one causes plants to grow in glass vessels containing the maximum quantity of vapour which the atmosphere can hold at the temperatures under trial, their unusual increase in dimensions will present a strikingly remarkable phenomenon.

503. In fourteen days from the commencement of this experiment another examination was made.

No. 1, all its leaves of a grass green.

No. 2, of a darker green.

No. 3, green, but of a bluish tint when compared with a plant taken from the garden.

No. 4, of a bright green.

No. 5, pale whitish yellow, with no fresh leaves, but grown to thirteen times its former height, and apparently in a vigorous condition.

With respect to No. 4, the plant under sulphocyanate of iron, I was not aware, at the time of making this trial, of the singular properties of that substance in relation to light; in the course of a fortnight, which had elapsed, the solution, from being of a deep blood red, had become perfectly colourless. No reliance is, therefore, to be placed on this result.

504. Among a number of experiments which were instituted with an intention of illustrating the same point, and which gave analogous results, it may be mentioned that the seeds of common garden cress were caused to germinate and grow in the boxes mentioned above; and no matter what was the substance through which the light passed, the young plants, after reaching a certain size, were always green, but those which grew in the dark had yellow leaves and white stalks.

505. The general result of these trials goes to prove that it is not this or that species of ray which gives rise to the colour of leaves; the absence of the chemical ray, or of the calorific ray, does not appear to affect it, nor have we any direct proof that the calorific ray exercises any influence. Humboldt has stated that in the mines of Germany, plants, as the *poa annua, et compressa, plantago lanceolata*, &c., grow in recesses where the sun's light never comes, and, provided hydrogen gas be present, their colour is green. In the Atlantic Ocean he saw a marine plant, *fucus vitifolius*, brought up from the depth of 190 French feet, where, according to the calculations of Bouguer, the light was only equal to that emitted from a candle at 203 feet distance, and yet its colour was green. Decandolle mentions that artificial light, as that of lamps, gives the same result; a proof that it is certainly not the chemical, and, perhaps, not the calorific rays, which cause the phenomenon.

506. Perhaps light, in this case, acts only as a kind of stimulus; it would be desirable to make trial of some plants whose leaves are naturally white; of this class there are several individuals; would they or would they not cause the decomposition of carbonic acid? From many indications, it is not improbable that there is a variety of chemical rays, each of which brings about changes of a character appropriate to itself. As yet we have not learned to distinguish these from each other, and are not provided with

the means of effecting their separation. A remarkable observation, which appears to me to be very much in point, was made many years ago by Professor SILLIMAN; it has not obtained that attention which it deserves; he states, that on exposure of a mixture of chlorine and hydrogen to the *light* of a fire, an explosion was produced. I quote the fact, however, only from memory, and have endeavoured to substantiate it under a variety of circumstances, but with a want of success probably due to the absorbing action of the glass jars used, or to the nature of the light. It is desirable that this experiment should be once more repeated; it would settle an important point—that chemical rays of different characters exist. I have referred to this before, for it is more than probable that there are chemical rays not absorbable by the chromates of potassa.

Note added to the foregoing Chapter.

(*Being a Letter to the Editors of the London and Edinburgh Philosophical Magazine, inserted in that Journal February*, 1840.)

AN ACCOUNT OF SOME EXPERIMENTS MADE IN THE SOUTH OF VIRGINIA ON THE LIGHT OF THE SUN.

507. GENTLEMEN—I have just seen in the Journals for the current month a letter from Sir J. HERSCHEL to the British Association for the Advancement of Science, in reference to some remarkable actions of the different colours of the solar spectrum.

508. About five years ago, having the advantage of a bright and almost tropical sky, I amused myself with attempting a repetition of MORICHINI's experiment for the magnetizing of steel, and was led to some results in respect to the chemical action of the sun's rays, which appear to bear very much on the subject of the letter above alluded to. Most of these have been published in the Journal of the Franklin Institute of Philadelphia; but as they do not appear to have been noticed in England, I will ask the favour of a page or two of your excellent Magazine, to give my testimony on a subject which now appears to excite so much interest.

509. If you pass a beam of the sun's light through a solution of chromate of potassa, it can no longer blacken a piece of sensitive paper; if you converge the light which has thus passed through a stratum of this fluid, by means of a lens, chloride of silver will remain for a long time, without much change, in the focus.

510. The list which was published in the Journal above named of solutions possessing this power, is as follows:

Bichromate of potassa.	Muriate of iron.
Chromate of potassa.	Chloride of gold.
Yellow hydro-sulphuret of ammonia.	Chloride of platinum.
Hydro-sulphuret of lime.	

511. It is to be remarked, that every one of these solutions is *yellow*, but I also found that a great many *vegetable coloured infusions* would, in like manner, absorb the chemical rays, especially those which have a *yellow* tint.

512. When I exposed pieces of paper covered with a layer of chloride of silver

to a beam which had passed through the red sulpho-cyanate of iron, the paper became of a brick-red colour; if to a beam which had passed through a solution of sulphate of copper and ammonia, it became of a blue-brown; and, lastly, on exposing a piece in a box, which I shall presently mention, for five days, to light which had been acted on by bichromate of potassa, it became perceptibly of a faint yellowish green.

513. It is very probable that there exist in the sunlight, rays having particular chemical powers.

514. A beam which has passed through bichromate of potassa does not appear to cause the union of a mixture of chlorine and hydrogen. I kept such a mixture for several hours in it, and could not perceive any change.

515. But this same beam can, nevertheless, enable vegetable leaves to effect the decomposition of carbonic acid. I took a wooden box, about a cubic foot in dimensions, and having removed its bottom, replaced it with a pair of parallel plates of glass, so adjusted that there was an interstice between them of half an inch, or thereabout. Into the trough thus formed I poured a solution of bichromate of potassa, or any other salt under trial, and the box being raised on one end, served as a closet in which bodies could be exposed to the action of beams that had passed through any given medium.

516. In this little chamber, its trough being filled with a solution of the bichromate, I placed a matrass containing water slightly impregnated with carbonic acid, and a few vegetable leaves; after a little while, air bubbles were copiously given off; there had been placed, similar in all respects, another matrass in the direct rays of the sun, and when a quantity of gas sufficient for analysis was evolved, it was found that carbonic acid had in both cases been decomposed, though, as might have been expected, in the latter more energetically. The result gave a mixture of carbonic acid, oxygen, and nitrogen: the uniform appearance of this latter body was subsequently traced to the leaves employed.

517. Plants also become green in light that has been submitted to the action of these yellow salts, and, therefore, deprived of the rays that blacken chloride of silver. I took a number of pea plants out of the garden, in May, 1837, and caused them to vegetate in light modified in this way, and also in light which had passed through sulpho-cyanate of iron, and sulphate of copper and ammonia, &c., but in every instance the leaves became green. It may also be mentioned that seeds of common cress were caused to germinate and grow under these circumstances; the young plants, after reaching a certain size, were always green, but those which had grown in the dark had yellow leaves and white stalks.

518. Professor Silliman states, in one of the early numbers of his Journal, that he witnessed an explosion of hydrogen and chlorine caused by the light of a common fire.

519. Ritter was the first who asserted that the opposite extremities of the spectrum possess opposite powers of chemical action; he states that phosphorus will emit fumes in the red ray, but if the violet be thrown on it, it ceases to smoke; this experiment I repeated often, and under favourable circumstances, but could not make it succeed.

520. I could succeed, however, in showing very beautifully the interference of that class of chemical rays which blacken chloride and bromide of silver, but failed in trying

to produce their polarization for want of proper apparatus. An electric current circulating in a wire does not seem to have any influence on these chemical rays; I found that the same neat magnified image of the wire was obtained on chloride paper when it was placed in a beam diverging from a lens, whether the current was made to pass or was stopped.

521. So much for chemical actions: let me now ask your attention to a mechanical result of solar light, which is very curious.

(*a.*) Having made a large air-pump jar very clean and dry, place a few pieces of camphor on the plate of the pump, and exhaust. Carry the pump with its receiver into the sunshine, and very soon you will see all that side which is nearest to the sun covered with crystals, but there will be few or none on the side which is farthest from him. With the brilliant sun of Virginia I have seen this effect take place, and beautiful stellated crystals appear *in four minutes*, literally covering the whole of the upper parts of the jar nearest the sun.

(*b.*) Or make a tube of half an inch or more in diameter, and upward of thirty inches long, a torricellian vacuum; pass up through the mercury a fragment of camphor. The tube may now be kept for any length of time in the dark without anything happening; but bring it into the beams of the sun, and in a few minutes crystallization will happen on the side next the luminary.

(*c.*) Again, paste on the inside of an air-pump jar a piece of tin foil an inch in diameter, and having operated as in experiment (*a*), expose this side towards the sun. Crystals will soon form, but the tin foil will protect the glass in its vicinity, and none will be found within a certain space round the metallic circle.

(*d.*) Crystallization is not necessarily connected with these results: the vapour of mercury in a torricellian void is condensed towards the light; so, also, the dew which settles on the inside of a jar containing water is always on the side nearest the window. The rays of the sun have also the power of decomposing a solution of chloride of gold: the metalline spangles are deposited on that side of the glass which is nearest to the light.

Artificial light gives none of these results.

(*e.*) Having removed the piece of tin foil used in experiment (*c*), place it on a little stand in front of the receiver; it will hinder the crystallization taking place in the parts on which its shadow is cast, and also for a certain space in the vicinity.

(*f.*) Take a jar that has already been coated with crystals, place the tin foil before it, and it will remove all those crystals which are within its shadow.

(*g.*) Instead of using a piece of tin foil, as in experiment (*c*), make the receiver hot, and rub upon it a piece of resin, so as to leave a transparent circle of that substance; expose to the light, and it will be found that the resin cannot protect the glass.

(*h.*) If along the inside surface of a vessel about to be exposed to the sun a glass rod be rubbed, rows of crystals will be deposited on the lines which were described by the end of the rod, but the vessel must be very dry for this experiment to succeed.

522. Now can we explain these singular results on any other *known* principle than this: That the side of the jar nearest the sun radiates freely the heat that it receives, back again, while radiation is interfered with at the other side; that, in point of fact, the anterior side is the colder, and the other the hotter?

CHAPTER XI.

ON THE PROCESS OF DAGUERREOTYPE, AND ITS APPLICATION TO TAKING PORTRAITS FROM THE LIFE.

Historical Note.—This chapter contains the first published description of the process of taking Daguerreotype portraits. Of late, both in America and in Europe, this art has been much cultivated and improved; it now forms a branch of industrial occupation. That it was possible by photogenic processes, such as the Daguerreotype, to obtain likenesses from the life, was first announced by the author of this volume in a note to the editors of the Philosophical Magazine, dated March 31st, 1840, as may be seen in that Journal, June, 1840, page 535. The first Daguerreotype portraits to which allusion is made in the following chapter (523) were produced in 1839, almost immediately after M. Daguerre's discovery was known in America.

It may farther be remarked, that of those spectral images which have excited so much attention of late in Europe, under the name of Moser's images, an account is here given, and given in connexion with the explanation of the Daguerreotype (527–544). A successful attempt was made in Germany, in 1842, to appropriate the discovery of these singular phenomena by the natural philosopher whose name now stands in connexion with them.

Of these two incidents in the science of photography, some account may be seen in the Edinburgh Review for January, 1843. In that work, the discovery of the art of taking Daguerreotype portraits, and the first observations on spectral images, are attributed to their true source, the author of this book.*

* "He was the first, we believe, who, under the brilliant summer sun of New-York, took portraits with the Daguerreotype. This branch of photography seems not to have been regarded as a possible application of Daguerre's invention, and no notice is taken of it in the reports made to the legislative bodies of France. We have been told that Daguerre had not, at that period, taken any portraits; and when we consider the period of time, twenty or twenty-five minutes, which was then deemed necessary to get a Daguerreotype landscape, we do not wonder at the observation of a French author, who describes the taking of portraits as *toujours un terrain un peu fabuleux pour le Daguerreotype.* Daguerre, however, and his countryman, M. Claudet, have nobly earned the reputation of having perfected this branch of the art.

"It has been long known that if we write upon a piece of glass with a pencil of soapstone or agalmatolite, the written letters, though wholly invisible, may be read by simply breathing upon the glass; and this even though the surface has been well cleaned after the letters had been written. Dr. Draper observed that if a piece of metal, a shilling, for example, or even a wafer, is laid upon a cool surface of glass or polished metal, and the glass or metal breathed upon, then, if the shilling is tossed from the surface, and the vapour dried up spontaneously, a spectral image of the shilling will be seen by breathing again upon the surface, the vapour depositing itself in a different manner upon the part previously protected by the shilling. More recently, Professor Draper has shown that this spectral image could be revived during a period of several months of the cold weather in the winter of 1840–41; but he has stated that he cannot find the reason of this result, though he regards it as analogous to the deposition of mercurial vapour in the Daguerreotype. We have often repeated this interesting experiment by keeping the protecting body, the shilling or wafer, *at a distance* from the glass or metallic surface, or by putting it under a watch-glass; and we found that the result was always the same (even after cleaning the surface with soft leather), so that change of temperature, or any pressure upon the glass surface, were excluded as causes of the phenomena."—(*Extract from the Edinburgh Review for January,* 1843, p. 339.)

(From the London and Edinburgh Philosophical Magazine for September, 1840.)

CONTENTS: *Daguerreotype Portraits from the Life first taken.—Spectral Images.—Preservation of the Sensitive Plate increases the Sensitiveness.—Modifications in the Daguerreotype Process.—Moonlight, Artificial Light, and Drummond's Light, are all active.—Description of the original Process of taking Portraits from the Life.*

523. VERY soon after M. DAGUERRE's remarkable process for photogenic drawing was known in America, I made attempts to accomplish its application to the execution of portraits from the life. M. ARAGO had already stated, in his address to the Chamber of Deputies, that M. DAGUERRE expected, by a slight advance, to meet with success, but as yet no account has reached us of that object being attained.

524. More than one hundred instances are recorded in BERZELIUS's chemistry, in which the agency of light brings about changes in bodies; these are of all kinds: formations of new compounds, rearrangements of elements already in union, changes of crystallographic character, decompositions, and mechanical modifications.

525. The process of the Daguerreotype is to expose a surface of pure silver to the action of the vapour of iodine, so as to give rise to a peculiar iodide of silver, which, under certain circumstances, is exceedingly sensitive to light. The different operations of polishing, washing with nitric acid, exposure to heat, &c., are only to offer a pure silver surface; the operation of hyposulphite of soda, and the process, which I shall presently describe, of galvanization, are to free the plate from its sensitive coating, and in nowise affect the depth of the shadows, as some of the French chemists at first supposed.

526. There is but one part of the Daguerreotype which does not yield to theory: on one point alone there is obscurity. Why does the vapour of mercury condense in a white form on those portions of the film of iodide which have been exposed to the influence of light? condense to an amount which is rigidly proportional to the quantity of incident light?

527. Even on this point there are facts which appear to have a bearing

(*a.*) It has long been known that if a piece of soapstone or agalmatolite be made use of as a pencil to write with on glass, though the letters that may have been formed are invisible, and though the surface of the glass may subsequently have been well cleaned, yet they will come into view as soon as the glass is breathed on.

(*b.*) I have often noticed that if a piece of very clear and cool glass, or, what is better, a cold polished metallic reflector, has a little object, such as a piece of metal, laid upon it, and the surface be breathed over once, the object being then carefully removed, as often as you breathe again on the surface, a spectral image of it may be seen, and this singular phenomenon may be exhibited for many days after the first trial was made.

(*c.*) Again, in the common experiment of engraving on glass by hydrofluoric acid, if the vapour has been very weak, no traces will be perceived on the glass after the wax has been removed; but, on breathing over it, the moisture condenses in such a way as to bring all the object into view.

(*d.*) In (491), (521) I have described a phenomenon which relates to the crystalli-

zation of camphor on surfaces of dry glass, on which invisible traces have been made by the pressure of a glass rod; this also appears to belong to the same class of effects.

528. BERZÈLIUS (Traité, vol. ii., p. 186) has attempted to explain (*a*) and (*c*) on this principle, that the changed and unchanged surfaces radiate heat unequally. There may be strong doubts with some as to the correctness of this, but is not the Daguerreotype due to the same cause, whatever it may be?

529. We must separate carefully the chemical changes which iodide of silver undergoes in the sunbeam from the mechanical changes which happen to the sensitive film: iodide of silver turns black in the solar ray; the whole success of the Daguerreotype artist depends on his checking the process before that change shall have supervened.

530. The coating of iodine is not *immediately* necessary to the production of images by the mercurial vapour. The condition seems to be traceable to the metallic surface. If you take a Daguerreotype, clean off the mercury, polish the plate thoroughly with rottenstone, wash it with nitric acid, and bring it to a brilliant surface, yet, if it has not been exposed to heat, the original picture will reappear on exposure to the mercurial vapour. Is not this a result of the same kind as those just referred to?

531. As a polishing material for the Daguerreotype plate, common rottenstone and oil answer very well. The plate having been planished by the workman, is to be rubbed down to a good surface, and as high a polish given to it as possible; it is to be heated and washed with nitric acid, as indicated in the French account, and finished by being rubbed with whiting (*creta præparata*), in the state of a very dry powder, going over it for the last time with a piece of clean dry cotton; this gives an intensely black lustre, which cannot be obtained by rottenstone alone, and thoroughly removes any film which nitric acid may have left.

532. To coat with iodine, I make use of a box about two inches deep, in the bottom of which that substance in coarse flakes is deposited; no cloth intervenes, but the silvered plate, with a temporary handle attached to it, is brought within half an inch of the crystals, and it becomes perfectly coated in the course of from one to three minutes; no metallic strips are necessary to ensure this effect; if the edges and corners are thoroughly clean, the golden hue will appear uniformly.

533. M. DAGUERRE recommends that the plate, after being iodized, shall be placed in the camera without loss of time. The longest interval, he says, ought not to exceed an hour. "Beyond this space the action of the iodine and silver no longer possesses the requisite photogenic properties."

534. There may be something peculiar in the preparation of the plate as I have described it, but it is certain that this observation must be received with some limitation. A plate which has been iodized does not appear so quickly to lose its sensitiveness. On the other hand, by keeping it in the dark for twelve or twenty-four hours, its sensitiveness is *often remarkably increased.* Other advantages also accrue. Those who have made many of these photogenic experiments, will have had frequent occasion to remark that the film of iodine is not equally sensitive all over; that there are spots or cloudy places which do not evolve any impression; and often the whole is in that condition, that the bright parts alone come out, while the parts that are in shadow

do not evolve correspondingly, nor can they be well developed, except at the risk of solarizing the picture. Now a plate that has been kept for several hours is by no means so liable to these effects: I do not pretend to give any reason for this, but merely mention it as a fact of considerable importance to the travelling Daguerreotyper; he will find that the iodine does not lose its sensitiveness in many days.

535. In a paper read before the Royal Society, of which an abstract is given in the April number of this Journal for the present year (p. 333), Sir John Herschel states that there is an absolute necessity of a perfect achromaticity in the object-glass of a photographic camera. M. Daguerre appears to have been under the same impression, and recommends in his published account such an object-glass.

536. All the rays of light, with, *perhaps*, the exception of the yellow, leave an impression on the iodide of silver. The less refrangible rays, however, act much more slowly than those which are at the opposite end of the spectrum. In the common kinds of glass, the most energetic action takes place in the indigo, or on the boundary of the blue. Now the retina receives an impression with equal facility from each of the different rays, the yellow light acting as quickly upon it as the red or the blue. Vision is therefore performed independently of time, the eye catching all the colours of the spectrum with equal facility and with equal speed. But it is not so with these photogenic preparations. In the action of light upon them, time enters as an element; the blue ray may have effected its full change, while the red is yet only beginning slowly to act; and the red may have completed its change before the yellow has made any sensible impression. On these principles, it is plain that an achromatic object-glass is by no means essential for the production of fine photographs; for if the plate be withdrawn at a certain period, when the rays that have a maximum energy have just completed their action, those that are more dispersed, but of slower effect, will not have had time to leave any stain. We work, in fact, with a temporary monochromatic light.

537. Upon these principles I constructed the camera which I am in the habit of using, with a double convex non-achromatic lens. Some of the finest proofs were procured with a common spectacle lens, of fourteen inches focus, arranged at the end of a cigar box as a camera; a lens of this diameter answers very well for plates four inches by three, reproducing the objects with the most admirable finish; copperplate engravings being represented in the minutest particulars, and the marks of the tool becoming quite distinct under the magnifier.

538. In this instance, it is true, owing to the magnitude of the focal length compared with the aperture, but little difficulty ensues from chromatic aberration; but when with the same focal length the aperture is increased to three or four inches, then the dispersion becomes very sensible, and yet good proofs can be procured by working in the method here indicated, the chief difficulty then arising from spherical aberration.

539. It has already been stated that the ray of maximum action for the Daguerreotype, when colourless French plate-glass is used, lies probably within the indigo space; it therefore follows that the length of the camera should be diminished, after arranging it to the luminous focus. The importance of this is pointed out in a paper by Mr. Towson, inserted in this Journal last year; I was, however, in the habit of using this

adjustment before reading the suggestions contained in that excellent communication. The amount of shortening which should be given to the camera, where the lens is fifteen inches focus, does not commonly exceed three tenths of an inch. If the luminous focus be used, the proof comes out indistinct.

540. In the subsequent process of mercurializing, it is of little importance what is the angular position. Several experimenters were for a time under the idea that an angle of 45° or 48° was a necessary inclination, in order that the plate should take the vapour; this arose from a misinterpretation of the printed account. Plates mercurialize equally well in a horizontal as in any other position; perhaps a slight inclination may be of advantage, in allowing the vapour to flow with uniformity over the iodized surface, but the chief use of an angle of 45° is to allow the operator to inspect the process through the glass.

541. Sometimes it is advantageous to heat the mercury a second time, when the proof is not distinctly evolved at first. Indeed, it occasionally happens, that a proof which did not evolve at all at first, will come out quite fairly on raising the temperature of the mercury again.

542. M. Daguerre recommends two methods of removing the sensitive coating from the plate: by washes of hyposulphite of soda, and a solution of common salt. The former answers perfectly, the second only indifferently well. There is, however, another process, which is very simple, and has an advantage over the former of these in cheapness. It adds not a little to the magic of the whole operation, in the eyes of those who are unaccustomed to chemical results. The plate, having been dipped into cold water, is placed in a solution of common salt, of moderate strength; it lies without being acted upon at all; but if it be now touched on one corner with a piece of zinc which has been scraped bright, the yellow coat of iodide moves off like a wave and disappears. It is a very pretty process. The zinc and silver forming together a voltaic couple, with the salt water intervening, oxidation of the zinc takes place, and the silver surface commences to evolve hydrogen gas; while this is in a nascent condition it decomposes the film of iodide of silver, giving rise to the production of hydriodic acid, which is very soluble in water, and hence instantly removed.

543. This process, therefore, differs from that with hyposulphite. The latter acts by dissolving the iodide of silver, the former by decomposing it. It is necessary not to leave the zinc in contact too long, or it deposits stains, and in large plates the contact should be made at the four corners successively, to avoid this accident.

544. After the proof is washed, all the defects in the preparation of the plate become apparent. If a film of mercury has existed on it, due to its not having been burned sufficiently long, there will be found a want of distinctness in the shadows; or if the plate has not been burned at all, perhaps the former impressions which have been obtained will reappear. This accident frequently happened in my earlier trials, when care had not been taken to give a due exposure each time to the spirit flame. Spectral appearances of former objects, on different parts of it, emerged—an interior with Paul Pry coming out, when the camera had been pointed at a church.

545. There is no difficulty in procuring impressions of the moon by the Daguerreo-

type, beyond that which arises from her motion. By the aid of a lens and a heliostat, I caused the moonbeams to converge on a plate, the lens being three inches in diameter. In half an hour a very strong impression was obtained. With another arrangement of lenses I obtained a stain nearly an inch in diameter, and of the general figure of the moon, in which the places of the dark spots might be indistinctly traced.

546. An iodized plate, being exposed for fifteen seconds only close to the flame of a gas light, was very distinctly stained; in one minute there was a very strong impression.

547. On receiving the image of a gas light, which was eight feet distant, in the camera, for half an hour, a good representation was obtained.

548. The flame of a gas lamp was arranged within a magic lantern, and a portion of the image of a grotesque on one of the slides received on a plate; a very good representation was procured.

549. With DRUMMOND's light, and the rays from a lime-pea in the oxy-hydrogen blowpipe, the same results were obtained.

550. In the first experiments which I made for obtaining portraits from the life, the face of the sitter was dusted with a white powder, under an idea that otherwise no impression could be obtained. A very few trials showed the error of this; for even when the sun was only dimly shining, there was no difficulty in delineating the features.

551. When the sun, the sitter, and the camera are situated in the same vertical plane, if a double convex non-achromatic lens of four inches diameter and fourteen inches focus be employed, perfect miniatures can be procured, *in the open air*, in a period varying with the character of the light, from 20 to 90 seconds. The dress, also, is admirably given, even if it should be black; the slight differences of illumination are sufficient to characterize it, as well as to show each button, button hole, and every fold.

552. Partly owing to the intensity of such light, which cannot be endured without a distortion of the features, but chiefly owing to the circumstance that the rays descend at too great an angle, such pictures have the disadvantage of not exhibiting the eyes with distinctness, the shadow from the eyebrows and forehead encroaching on them.

553. To procure fine proofs, the best position is to have the line joining the head of the sitter and the camera so arranged as to make an angle with the incident rays of less than ten degrees, so that all the space beneath the eyebrows shall be illuminated, and a slight shadow cast from the nose. This involves obviously the use of reflecting mirrors to direct the ray. A single mirror would answer, and would economize time, but in practice it is often convenient to employ two: one placed, with a suitable mechanism, to direct the rays in vertical lines; and the second above it, to direct them in an invariable course towards the sitter.

554. On a bright day, and with a sensitive plate, portraits can be obtained in the course of five or seven minutes in the diffused daylight. The advantages, however, which might be supposed to accrue from the features being more composed, and of a more natural aspect, are more than counterbalanced by the difficulty of retaining them so long in one constant mode of expression.

555. But in the reflected sunshine, the eye cannot support the effulgence of the rays. It is, therefore, absolutely necessary to pass them through some blue medium, which shall abstract from them their heat, and take away their offensive brilliancy. I have used for this purpose blue glass, and also ammoniaco-sulphate of copper, contained in a large trough of plate glass, the interstice being about an inch thick, and the fluid diluted to such a point as to permit the eye to bear the light, and yet to intercept no more than was necessary. It is not requisite, when coloured glass is employed, to make use of a large surface; for if the camera operation be carried on until the proof *almost* solarizes, no traces can be seen in the portrait of its edges and boundaries; but if the process is stopped at an earlier interval, there will commonly be found a stain corresponding to the figure of the glass.

556. The camera I have used, though much better ones might be constructed, has for its objective two double convex lenses, the united focus of which for parallel rays is only eight inches; they are four inches in diameter in the clear, and are mounted in a barrel, in front of which the aperture is narrowed down to $3\frac{1}{2}$ inches, after the manner of DAGUERRE'S.

557. The chair in which the sitter is placed has a staff at its back, terminating in an iron ring, that supports the head, so arranged as to have motion in directions to suit any stature and any attitude. By simply resting the back or side of the head against this ring, it may be kept sufficiently still to allow the minutest marks on the face to be copied. The hands should never rest on the chest, for the motion of respiration disturbs them so much as to bring them out of a thick and clumsy appearance, destroying also the representation of the veins on the back, which, if they are held motionless, are copied with surprising beauty.

558. It has already been stated that certain pictorial advantages attend an arrangement in which the light is thrown upon the face at a small angle. This also allows us to get rid entirely of the shadow from the background, or to compose it more gracefully in the picture; for this, it is well that the chair should be brought forward from the background from three to six feet.

559. Those who undertake Daguerreotype portraitures will, of course, arrange the backgrounds of their pictures according to their own tastes. When one that is quite uniform is desired, a blanket or a cloth of a drab colour, properly suspended, will be found to answer very well. Attention must be paid to the tint: white, reflecting too much light, would solarize upon the proof before the face had had time to come out, and owing to its reflecting *all* the different rays, a blur or irradiation would appear on all edges, due to chromatic aberration. It will be readily understood that if it be desired to introduce a vase, an urn, or other ornament, it must not be arranged against the background, but brought forward until it appears perfectly distinct on the obscured glass of the camera.

560. Different parts of the dress, for the same reason, require intervals, differing considerably, to be fairly copied, the white parts of a costume passing on to solarization before the yellow or black parts have made any decisive representation. We have, therefore, to make use of temporary expedients. A person dressed in a black coat, and

open waistcoat of the same colour, must put on a temporary front of a drab or flesh-colour, or by the time that his face and the fine shadows of his woollen clothing are evolved, his shirt will be solarized, and be blue, or even black, with a white halo around it. Where, however, the white parts of the dress do not expose much surface, or expose it obliquely, these precautions are not essential; the white shirt collar will scarcely solarize until the face is passing into the same condition.

561. Precautions of the same kind are necessary in ladies' dresses, which should not be selected of tints contrasting strongly.

562. It will now be readily understood that the whole art of taking Daguerreotype miniatures consists in directing an almost horizontal beam of light, through a blue-coloured medium, upon the face of the sitter, who is retained in an unconstrained posture by an appropriate but simple mechanism, at such a distance from the background, or so arranged with respect to the camera, that his shadow shall not be copied as a part of his body; the aperture of the camera should be three and a half or four inches at least; indeed, the larger the better, if the object glass be aplanatic.

563. If two mirrors be made use of, the time actually occupied by the camera operation varies from forty seconds to two minutes, according to the intensity of the light. If only one mirror is employed, the time is about one fourth shorter. In the direct sunshine, and out in the open air, the time varies from under half a minute.

564. Looking-glasses which are used to direct the solar rays, after a short time undergo a serious deterioration; the foil assuming a dull granular aspect, and losing its black brilliancy. Hence the time in copying becomes gradually prolonged.

565. The arrangement of the camera, above indicated, gives reversed pictures, the right and left sides changing places. Mr. Woolcott, an ingenious mechanician of this city, has taken out a patent for the use of an elliptical mirror for portraiture; it is about seven inches in aperture, and allows him to work conveniently with plates two inches square. The concave mirror possesses this capital advantage over the convex lens, *that the proof is given in its right position, that is to say, not reversed;* but it has the serious inconveniences of limiting the size of the plate, and representing parts that are at all distant from the centre in a very confused manner. With the lens, plates might be worked a foot square, or even larger.

566. Miniatures procured in the manner here laid down are in most cases striking likenesses, though not in all. They give, of course, all the individual peculiarities—a mole, a freckle, a wart. Owing to the circumstance that yellow and yellowish browns are long before they impress the substance of the Daguerreotype, persons whose faces are freckled all over give rise to the most ludicrous results, a white mottled with just as many black dots as the sitter had yellow ones. The eye appears beautifully; the iris with sharpness, and the white dot of light upon it, with such strength and so much of reality and life, as to surprise those who have never before seen it. Many are persuaded that the pencil of the painter has been secretly employed to give this finishing touch.

CHAPTER XII.

ON SOME ANALOGIES BETWEEN THE PHENOMENA OF THE CHEMICAL RAYS AND THOSE OF RADIANT HEAT.

(From the London, Edinburgh, and Dublin Philosophical Magazine for September, 1841.)

CONTENTS: *Object of the Memoir.—On the Daguerreotype Process.—Chemical Constitution of Daguerreotype Pictures.—Spectral Images.—Film of Iodide torn off Mechanically.—Iodine is not evolved, but corrodes the Plate.—The Chemical Rays are absorbed.— The Photographic Effects are transient.— The Chemical Rays are not conducted.— They become Latent.— Optical Qualities control Chemical Action.— The Active Rays are absorbed, and the Complementary reflected.—Relation of Optical Forces and Chemical Affinities.*

567. IT is the object of this memoir to establish some striking analogies which exist between the phenomena of the chemical rays and those of radiant heat.

568. As most of the experimental illustrations which I shall here give depend upon the use of M. DAGUERRE's preparation (though I have numerous others which serve to extend these truths to other combinations, and which will be published in due time), I shall also, incidentally, give what appears to be the proper theory of the Daguerreotype.

569. Without saying anything of the laws of reflexion, refraction, polarization, and interference, to which these rays are subject, the study of which I commenced more than five years ago on paper rendered sensitive by the bromide of silver, farther than that a general similitude holds in all these cases between the rays of heat and the chemical rays, I shall at present confine my observations to establishing the following propositions:

1st. That the chemical action produced by the rays of light depends upon the ABSORPTION of those rays by sensitive bodies; just as an increase of temperature is produced by the absorption of those of heat.

2d. That as a body warmed by the rays of the sun gradually loses its heat by radiation, or conduction, or contact with other bodies, so likewise, by some unknown process, photographic effects produced on sensitive surfaces are only transient, and gradually disappear.

3d. That, as when rays of heat fall on a mass of ice, its temperature rises degree by degree, until it reaches 32° Fah., and there stops, until a certain molecular change (liquefaction) is accomplished, and after that proceeds to rise again, so, also, the chemical rays impress certain changes proportional to their quantity, up to a certain point, and there a pause ensues; a very large amount of light being now rendered latent or absorbed, without any indication thereof being given by the sensitive preparation (as the heat of fluidity is latent to the thermometer), a molecular change then setting in, the increments of the quantity of light are again indicated by changes in the sensitive preparation.

4th. That it depends on the CHEMICAL nature of the ponderable material what rays shall be absorbed.

5th. That while the *specific rays* thus absorbed depend upon the chemical nature of the body, the *absolute amount* is regulated by its OPTICAL qualities, such as depend on the condition of its surfaces and interior arrangement.

6th. It will be proved from this, that the SENSITIVENESS of any given substance depends on its chemical nature and optical qualities conjointly, and that it is possible to exalt or diminish the sensitiveness of any given chemical compound, by changing the character of its optical relations. We shall here meet with an explanation of some of the facts noticed by Sir J. HERSCHEL, Mr. HUNT, Mr. TALBOT, and others, respecting the increase of sensitiveness of the chloride of silver and other bodies.

7th. That, as when radiant heat falls on the surface of an opaque body, the rays reflected are complementary in number to those that are absorbed, so, in the case of a sensitive preparation, the rays reflected are complementary in number to those that are absorbed.

570. OF THE DAGUERREOTYPES.—In relation to the condition of these tablets, I shall prove the following facts:

1st. That metallic mercury exists all over the surface of an ordinary Daguerreotype, in the shadows as well as in the lights; in the shadows it is as metallic mercury, in the lights as silver amalgam.

2d. That in an iodized Daguerreotype, as taken from the mercury-bath, there is no order of superposition of the parts, that is to say, the iodide is neither *upon* nor *beneath* the mercury, but both are, as it were, in the same plane.

3d. That when a ray of light falls upon the surface of this preparation, through all the intervening steps, and up to the point of maximum action, no iodine is evolved from the plate; but that in the common Daguerreotype the light communicates a tendency to the atoms of the iodide to yield up to the mercurial vapour their silver, while the iodine retires and combines with the unaffected silver around. It follows that when such a plate is withdrawn from the mercurial vapour, there is all over it a uniform film of iodide of silver of the very same thickness as at first; and this has happened through a *direct corrosion* of the silver by the iodine, while it was undergoing the mercurial operation.

571. I pass at once to the proofs of these several propositions, commencing, for the sake of perspicuity, with those relating to the Daguerreotype first: and 1st, *That metallic mercury exists all over the surface of an ordinary Daguerreotype, in the shadows as well as in the lights; in the shadows it is as metallic mercury, in the lights as silver amalgam.*

572. I took a plated copper three inches by four in surface, and having prepared it with care, I exposed half of it to the diffused light of the day, screening the other half; it was then mercurialized at 175° Fah., the iodide removed by hyposulphite of soda and washed. And now a plate on which a gold leaf was spread was placed over it, but separated, as shown in *fig.* 94, in the points *a*, *b*, *c*, by three slips of glass. By means of a spirit-lamp the photographic plate *a*, *b*, *c*, was heated, and the gilded plate

g k kept cool, by occasionally wetting it. On parting the plates, it was perceived that faint but distinct traces of whitening were visible all over the gold, as well on that part which was over the whitened half of the photograph as over that which was unchanged.

573. But as it might happen that the mercury diffused itself laterally past the imperfect obstacle *b*, I made the following decisive trials:

I iodized three silver plates, A, B, C, each three inches by four in surface, conducting the processes for each in the same way; and having exposed each for two minutes to a faint daylight, I laid them aside in the dark, to be presently used as test plates, in lieu of the gilded plate *g k*.

Then I took three other plates, D, E, F, of the same size, and conducting the preparatory processes for each as before, I iodized D in the dark, and mercurialized it forthwith at 170° Fah., taking the utmost care that not a ray of light should be suffered to impinge upon it.

E was iodized, and exposed for two minutes to diffused daylight, and then mercurialized at 170° Fah.

F was iodized, and exposed to the sun until it began to turn brown, an effect occurring almost at once. It was then mercurialized at 170° Fah.

All these plates then had their sensitive coating removed by hyposulphite, and were thoroughly washed in distilled water and dried.

574. I had, therefore, three plates, representing accurately the conditions proposed to be investigated. D was in the condition of the most perfect shadows, E in that of the highest lights, and F solarized. In appearance D was black, E was white, and F bluish-gray.

Upon D, E, F, I placed A, B, C, respectively, separating each pair of plates one sixteenth of an inch, or thereabout, by slips of glass. Then I laid them on the level surface of the sand-bath, the test plates being kept cool by sponging occasionally with water. Temperature of the sand, 200° Fah.; duration of the experiment, fifteen minutes.

On examination, A, B, C were all found powerfully mercurialized, nor did there seem to be any difference between them.

575. I consider, therefore, that the shadows, the demitints, the lights, and the solarized portions of a Daguerreotype, are covered with mercury; for at a temperature of 200° Fah., they all evolve it alike, a sufficiency of vapour rising from the parts that have not been exposed to the light to bring a plate that has been so exposed to its maximum of whiteness.*

576. In (527) I described a remarkable effect which I had noticed in these investigations: that if an object, such as a wafer, be laid upon a piece of cold glass or metal, and you breathe once on it, and as soon as the moisture has disappeared, remove the object, and breathe again on the glass, a spectral image of the wafer will make its appearance. The impression thus communicated to the surface, under certain conditions, remains there for a long time. During the cold weather last winter, I produced

* I believe that the most delicate test for the presence of mercury is a slip of silver iodized to a yellow colour, and exposed for two or three minutes to a weak daylight.

such an image on the mirror of my heliostat; it could be revived by breathing on the metal many weeks afterward, nor did it finally disappear until the end of several months.

577. I do not at present know what is the reason of this result, but the analogy between it and the arrangement of mercurial globules, which cover the surface of a Daguerreotype, is too striking to be overlooked. It proves to us that surfaces may assume such a condition as to affect the deposition of vapours upon them, so as to give rise to the reproduction of appearances of external forms. I gave, therefore, particular attention to this point, but eventually found that silver exists in an ordinary Daguerreotype, in connexion with the mercury, all over the plate, in a less proportion in the shadows, and in a greater proportion in the lights. This result was, however, only obtained after the following fact was discovered: that the mucilage of gum-arabic, when slowly dried in a thin layer on the surface of a Daguerreotype, splits up in shivers, bringing along with it the white portions of the picture, and leaving the plate clean.

578. Having, therefore, prepared three plates, D, E, F, exactly as before (573), I poured on them a solution of gum, drained them so as to leave only a small quantity, and let them dry slowly over the sand-bath. The gum separated readily, and lay in chips on the surface of each plate; it was easily removed to three sheets of paper, by tapping with the finger on the back of the plate. Each was then treated alike, as follows:

579. The gummy matter was incinerated on a platinum leaf, and the remaining ashes transferred to a test tube, half an inch in diameter. One drop of nitric acid and one drop of water were added; it was boiled over a small flame, and diluted with a little water. Dilute muriatic acid was now added, and the chloride of silver immediately fell. In repeating this, it is necessary to attend to the state of dilution of the acid, for if too strong, it wholly dissolves the minute quantity of chloride of silver generated.

580. As, from the minuteness of that quantity, it was impossible to obtain a direct quantitative analysis, I adopted the foregoing method, and added the dilute acid to all three tubes at the same time. In D there was a faint opalescence, in E and F a cloud; but I could not always determine whether the deposite of E or F was most copious, sometimes the one and sometimes the other appearing to have a slight advantage.

581. I conclude, therefore, that while the whole surface of the plate is coated with mercury, it exists as silver amalgam chiefly in the lights, and as uncombined mercury chiefly in the shadows, and in a mixed proportion in the demitints; and that when a plate is solarized, both free mercury and amalgam are present.

582. Such is the state of surface in a Daguerreotype *recently formed.* In the course of time, however, a great portion of the mercury that is in the shadows, and also free in the lights, evaporates away. When the picture has thus changed, the shadows are metallic silver, and the lights silver amalgam.

583. 2d. *That in an iodized Daguerreotype, as taken from the mercury-bath, there is no order of superposition of the parts, that is to say, the iodide is neither upon nor beneath the mercury, but both are, as it were, in the same plane.*

Soon after I had ascertained the action of gum-arabic, some of it was applied to the surface of a plate on which an impression had just been formed in the mercury-bath. This was without removing the coat of iodine. On drying it, the gum chipped up, as was expected, bringing away with it all the lights of the picture, and leaving a uniform coat of yellow iodide of silver beneath. It seems, therefore, that the film of iodide coheres more strongly to the metal plate than the amalgam; and, farther, from this result we should judge that the amalgam is *on the surface* of the iodide.

584. But this is not true; for on three different occasions I have found that when Russian isinglass was employed instead of gum, for purposes presently to be related (591), the isinglass, from its stronger cohesive power, chipped off in the act of drying, tearing up the yellow film from end to end of the plate, and leaving the amalgam, constituting the lights, undisturbed. It is here to be understood that this action takes place without the smallest disturbance of the lights and demitints, the plate remaining in all the beauty, and brilliancy, and perfection that it would have had if it had been carefully washed in hyposulphite of soda.

585. This is a result, however, which I cannot produce with uniformity. Most commonly, the lights are torn up with the iodide. Had it occurred but once, I should still have cited it with decision, for, from the very character of it, it is impossible to be mistaken, or to commit an error of judgment. It proves to us that the film of iodide may be mechanically *torn off* from the metallic surface as perfectly as it can be *dissolved off* by chemical agents—a singular fact.

586. This result, therefore, proving that we can tear off the film of iodide and leave the amalgam, can only be co-ordinated with that (583) by gum-water, in which the amalgam is removed and the iodide left, by supposing that there is not anything like a direct superposition in the case, and that the particles of amalgam and iodide lie, as it were, side by side.

587. 3*d. That when a ray of light falls upon the surface of this preparation, through all the intervening steps, and up to the point of maximum action, no iodine is evolved from the plate, but that in the common Daguerreotype the light communicates a tendency to the atoms of the iodide to yield up to the mercurial vapour their silver, while the iodine retires, and combines with the unaffected silver around. It follows that, when such a plate is withdrawn from the mercurial vapour, there is all over it a uniform film of iodide of silver, of the very same thickness as at first; and this has happened through a direct corrosion of the silver by the iodine, while it was undergoing the mercurial operation.*

There is no difficulty in proving this directly, and the indirect evidence is copious. If we lay a piece of paper imbued with starch on an iodized plate, and expose it to the sun, although the plate presently assumes a dark olive-green colour, the starch remains uncoloured.

588. This dark substance is probably a subiodide of silver; the iodine, therefore, which has been disengaged from it not having been set free, must have necessarily united with the adjacent metallic silver: this, for very obvious reasons, there is no difficulty in admitting.

589. Now, therefore, when a photogenic impression existing on the surface of a plate in an invisible state is brought out by the action of mercury vapour, we easily understand how this is effected. No iodine is ever evolved. But each atom of iodide of silver that has been acted on by the light yields to the attraction of the mercury its atom of silver, and the iodine thus set free unites with the metallic silver particles around it, reproducing the same yellow iodide by *a direct corrosion* of the plate: the proofs that we have of this are two in number.

590. 1st. Dry some mucilage of gum-arabic on a Daguerreotype just brought from the mercury-bath; when it has split up, we perceive that the white amalgam of silver is removed, and a uniform coat of yellow iodide of silver, of the very same thickness as at first, as is proved by its colour, is left.

591. 2dly. Dry upon the same plate a solution of Russian isinglass, and, when it has split up, it will be seen that it uniformly rends off with it the yellow iodide, leaving the metallic plate with an exquisite polish; and wherever the light has touched, *there it is corroded.*

592. These two facts, taken together, prove that in mercurializing a plate no iodine is evolved, but that a new film of iodide of the same thickness is formed, at the expense of the metallic surface.

593. From these facts we readily gather that on the presence of the metallic silver the sensitiveness of this preparation mainly depends, for to the tendency which the light has impressed on the elements of the iodide to separate is added the strong attraction of metallic silver for nascent iodine.

594. This corrosion or biting in of the silver plates, by the conjoint action of the mercury and iodine, gives rise to etchings that have an inexpressible charm. Could any plan be hit upon of forcing the iodiue to continue its action, the problem of producing *engraved* Daguerreotypes would be solved. By another process, which will be described hereafter, I have succeeded in producing deep etchings from Daguerreotypes.

595. I now commence with the proofs of the leading propositions set out with in this communication.

And, 1st. That the chemical action produced by the rays of light depends upon the ABSORPTION *of those rays by sensitive bodies, just as an increase of temperature is produced by the absorption of those of heat.*

596. Without embarrassing myself here with any considerations of the tints of thin plates, or the colours of natural objects, I shall use the term absorption as expressive of a loss of radiant matter, whether that loss arises from a direct union of the luminous molecules with ponderable matter, or is rather a disappearance of effect, caused by the interference of systems of undulations.

597. I iodized a plate to a golden-yellow colour, and exposed it to the diffused light of day, setting it in such a position that it reflected specularly the light falling upon it through the window to the objective of a camera-obscura, which formed an image of it upon a second sensitive plate. The beams falling upon the sensitive plate of course exerted their usual influence upon the iodide, which, after the lapse of a short

time, began to turn brown. As soon as this effect was observed, I closed the aperture of the camera, and, taking out its plate, mercurialized it; but it was found that the rays reflected from the sensitive plate, although they had been converged by a lens four inches in diameter, and formed a very bright image, *had lost the quality of changing the iodide of silver.*

598. We see, therefore, that a ray of light which has impinged on the surface of yellow iodide of silver, has lost the quality of causing any farther change on a second similar plate on which it may fall.

599. In the practice of photogenic drawing, this observation is of much importance, especially when lenses having large apertures are used; the rays which converge upon the sensitive plate are reflected by it in all directions, and the camera is full of light; its sides reflect back again in all directions on the surface of the plate these rays, which, if they were effective, must stain the plate in the shadows. But if the plate has been iodized to the proper tint, this light is wholly without action, and hence the proof comes out neat and clean.

600. Upon an iodized plate I received a solar spectrum formed by a flint-glass prism, the ray being kept motionless by reflexion from a heliostat, and the plate so arranged as to receive the refracted rays perpendicularly. After five minutes it was mercurialized, and the resulting proof exhibited the place of the more refrangible colours in the most brilliant hues. The lesser refrangible colours had also left their impress of a whitish aspect, but the region of the yellow was unaltered. All the different rays, therefore, except the yellow, have the power of changing this particular preparation. Now when a number of pieces of cloth of different colours are placed in the sunbeam, they absorb heat in proportion as their colour is deeper. A black cloth, which does not reflect any of those calorific rays, becomes presently *hot;* and in the same way DAGUERRE's sensitive preparation absorbs all the rays which have any chemical action on it, and reflects the yellow only, which does not affect it. In this particular lies the secret of its vast sensitiveness, compared with the common preparations of the chloride and bromide of silver.

601. *2d. That as a body warmed by the rays of the sun gradually loses its heat by radiation, or conduction, or contact with other bodies, so likewise, by some unknown process, photographic effects produced on sensitive surfaces are only transient, and gradually disappear.*

602. After a beam of light has made its impression on the iodide, if the plate be laid aside in the dark before mercurializing, that impression decays away with more or less rapidity; first the faint lights disappear, then those that are stronger.

603. Having brought three plates to the same condition of iodization, and received the image of a gas-flame in the camera on each for three minutes, I mercurialized one, A, forthwith; the second, B, I kept an hour, the third, C, forty-eight hours; the relative appearance of these three images is represented in *fig.* 95.

604. Those who are in the habit of taking Daguerreotypes know how much they suffer when the process of mercurialization is deferred. To show this effect in the extreme, I took four plates, and having prepared all alike, I exposed half of the surface of each to a bright sky for eight seconds.

No. 1, mercurialized	immediately,	came out	black solarized.
2, "	in five hours,	"	white.
3, "	twenty-two hours,	"	same effect.
4, mercurialized	one hundred and forty-four,		no effect.

605. This last plate, on being submitted twice more to the vapour of mercury, gave an indistinct mark. On exposing a corner of it to the sun, it blackened instantly; these results showing that the peculiar condition brought on by the action of the light gradually disappears, the compound all the time retaining its sensitiveness.

606. Similar results are mentioned by DAGUERRE in the case of the changes produced on surfaces of resinous bodies, and I have noticed them in a variety of other cases. Now to whatever cause these phenomena are due, whether to anything analogous to radiation, conduction, &c., it is most active during the first moment after the light has exerted its agency, but it must also take effect even at the very time of exposure; and it is for these reasons that it comes to pass, that when light of a double intensity is thrown upon a metallic plate, the time required to produce a given effect is less than one half.

607. I could conceive the intensity of a ray so adjusted, that in falling upon a given sensitive preparation, the loss from this cause, this casting off of the active agent, should exactly balance the primitive effect, and hence no observable change result. Hereafter we shall find that one cause of the non-sensitiveness of a number of bodies is to be traced directly to the circumstance that they yield up these rays as fast as they receive them.

608. It needs no other observation than a critical examination of the sharp lines of a Daguerreotype proof with a magnifying glass, to show that the influence of the chemical rays is not propagated laterally on the yellow iodide of silver. Of the manifestations which these rays may exhibit after they have lost their radiant form and become absorbed, we know but little. If they conform to the analogous laws for heat, and if the absorbing action of bodies for this agent is inversely as their conducting power, we perceive at once *why* a photographic effect produced on yellow iodide of silver retains the utmost sharpness without any lateral spreading; the absorbing power is almost perfect, the conducting should therefore be zero.

609. *3d. That, as when rays of heat fall on a mass of ice, its temperature rises degree by degree, until it reaches 32° Fah., and there stops, until a certain molecular change (liquefaction) is accomplished, and after that proceeds to rise again, so, also, the chemical rays impress certain changes proportional to their quantity, up to a certain point, and there a pause ensues; a very large amount of light being now rendered latent or absorbed, without any indication thereof being given by the sensitive preparation (as the heat of fluidity is latent to the thermometer), a molecular change then setting in, the increments of the quantity of light are again indicated by changes in the sensitive preparation.*

610. Although in the sun the iodide of silver blackens at once, this is only the result of a series of preliminary operations.

611. When we look at a Daguerreotype, we are struck with the remarkable gradation of tint, and we naturally infer that the amount of whitening induced by mercurialization is in direct proportion to the amount of incident light; otherwise it would hardly seem that the gradation of tones could be so perfect.

612. But, in truth, it is not so. When the rays begin to act on it, the iodide commences changing, and is capable of being whitened by mercury. Step by step this process goes on, an increased whiteness resulting from the prolonged action or increased brilliancy of the light, until a certain point is gained, and now the iodide of silver apparently undergoes no farther visible change; but another point being gained, it begins to assume, when mercurialized, a pale-blue tint, becoming deeper and deeper, until it at last assumes the brilliant blue of a watch-spring. This incipient blueness goes under the technical name of solarization.

613. The successful practice of the art of Daguerreotyping, therefore, depends on limiting the action of the sun-ray to the first moments of change in the iodide; for, if the exposure be continued too long, the high lights become stationary, while the shadows increase unduly in whiteness, and all this happens long before solarization sets in.

614. Let us examine these important phenomena more minutely. Having carefully cleaned and iodized a silver plate three inches by four in size, it is to be kept in the dark an hour or two.

615. By a suitable set of tin foil screens, rectangular portions of its surface, half an inch by one eighth, are to be exposed at a constant distance to the rays of an argand gas-burner (the one I have used is a common twelve-holed burner), the first portion being exposed fifteen seconds, the second thirty seconds, the third forty-five seconds, the fourth sixty seconds, &c., &c.

616. We have thus a series of discs or spaces upon the plate (*a, b, c, d, fig.* 96), each of which has been affected by known quantities of light; *b* being affected twice as much as *a*, having received a double quantity of light; *c* thrice as much as *a*, having received a triple quantity, &c., &c.

617. The plate now is exposed to the vapour of mercury at 170° Fah. for ten minutes; the spaces or discs all come out in their proper order, and nothing remains but to remove the iodine.

618. An examination of one of these plates thus prepared shows us* that, commencing with the first space *a*, we discover a gradual increase of whitening effect until we reach the seventh; that a perfect whiteness is there attained; that, passing on to the sixteenth, no increase of whitening is to be perceived, although the quantities of light that have been incident and absorbed have been continually increasing; but as soon as the light thus latent has reached a certain quantity, visible decomposition sets in, indicated by a blueness, and the sensitive surface once more renders evident the increments of incident light.

619. Or, by presenting a plate covered with a screen to a sky that is clear or uniformly obscured, and with a regular motion, withdrawing the screen deliberately from one end to the other, and then suddenly screening the whole, it is plain that those parts first uncovered will have received the greatest quantity of light, and the others less and less. On mercurializing, it will be seen that a stain will be evolved on the

* It is impossible to represent these changes in a drawing, which is simply black and white; it will be understood that the characteristic distinction of the spaces, from the sixteenth to the twentieth, for example, depends on their assuming a *blue* tint, which continually deepens in intensity.

plate, as is represented in *fig.* 97 ; from *a* to *b* the changes have been successive ; from *b* to *c* no variation in the amount of whitening is perceptible ; at *d* solarization is commencing, which becomes deeper and deeper to the end, *e*, of the stain.

620. The plate from which the drawing of *fig.* 97 is taken gives, from *a* to *b*, ten parts, from *b* to *c* seventeen parts, from *d* to *e* twelve parts ; we perceive, therefore, how large an amount of light is absorbed, and its effects rendered latent, between the maximum of whiteness being gained and solarization setting in.

621. 4th. *That it depends on the* CHEMICAL *nature of the ponderable material what rays shall be absorbed.*

I had prepared a number of observations in proof of this, very much of the same kind as those which have some time ago been published in the Philosophical Transactions by Sir J. HERSCHEL. These refer chiefly to the variable lengths of the stains, impressed by the prismatic solar spectrum on different chemical bodies, and the points of maximum action noticed in them. For the present, I content myself with referring to that excellent memoir for proofs substantiating this proposition.

622. 5th. *That while the specific rays thus absorbed depend upon the chemical nature of the body, the absolute amount is regulated by its* OPTICAL QUALITIES, *such as depend on the condition of its surfaces and interior arrangement.*

623. I took a polished silver plate, and, having exposed it to the vapour of iodine, found that it passed through the following changes of colour : 1st, lemon yellow ; 2d, golden yellow ; 3d, reddish yellow ; 4th, blue ; 5th, lavender ; 6th, metallic ; 7th, yellow ; 8th, reddish ; 9th, green, &c., &c., the differences of colour being produced by the differences of thickness in the film of iodide, and not by any difference of chemical quality.

624. It is a common remark, originally made by M. DAGUERRE, that of these different tints that marked 2 is the most sensitive, and photogenic draughtsmen generally suppose that the others are less efficient from the circumstance of the film of iodide being too thick. Some suppose, indeed, that the first yellow alone is sensitive to light. We shall see, in a few moments, that this is very far from being the case.

625. Having brought nine different plates to the different colours just indicated, I received on each the image of a uniform gas flame in the camera, treating all as nearly alike as the case permitted. I readily found that in No. 1 there was a well-marked action ; No. 2, still stronger, but that the rays had less and less influence down to No. 6, in which they appeared to be almost without action ; but in No. 7 they had recovered their original power, being as energetic as in No. 2, and from that declining again ; this is shown in *fig.* 98.

626. Hence we see that the sensitiveness of the iodide of silver is by no means constant ; that it observes periodical changes, which depend on the optical qualities of the film, and not on its chemical composition ; and that by bringing the iodide into those circumstances that it reflects the blue rays, we greatly reduce its sensitiveness, and still more so when we adjust its thickness so as to give it a gray metallic aspect. But the moment we go beyond this, and restore by an *increased* thickness *its original colour,*

we restore also its sensitiveness. Here, then, in this remarkable result, we again perceive a corroboration of our first proposition.

627. I may, however, observe in passing, that although I am describing these actions as if there was an actual absorption of the rays, and that films on metallic plates exhibit colours, not through any mechanism like interference, but simply because they have the power of absorbing this or that ray, there is no difficulty in translating these observations into the language of that hypothesis. When the diffracted fringes given by a hair or wire in a cone of diverging light are received on these plates, corresponding marks are obtained, a dark stripe occupying the place of a yellow fringe, and a white that of a blue. I found, more than four years ago, that this held in the case of bromide of silver paper (446), and have since verified in a more exact way with this French preparation. Similar phenomena of interference may be exhibited with the chloride of silver.

628. We have it, therefore, in our power to exalt or depress the sensitiveness of any compound by changing its optical conditions. Until now, it has been supposed that the amount of change taking place in different bodies by the action of the rays of light, depended wholly on their chemical constitution, and hence comparisons have been instituted as to the relative sensitiveness of the chlorides, bromides, oxides, and iodides of silver, &c. But it seems this liability to change depends also on other principles, which being liable to variation, the sensitiveness of a given body varies with them. Thus this very iodide of silver, when in a thin yellow film, is decomposed by the feeblest rays of a taper, and even moonlight acts with energy; yet simply by altering the thickness of its film, it becomes sluggish, blackening in the sunlight tardily, and recovers its sensitiveness again on recovering its yellow hue.

629. We have now no difficulty in understanding how, in the preparation of ordinary sensitive paper, great variations ensue by modifying the process slightly, and how, even on a sheet which is apparently washed uniformly over, large blotches appear which are either inordinately sensitive or not sensitive at all. If, without altering the chemical composition of a film on metallic silver, or even its mode of aggregation, such striking changes result by *difference of thickness*, how much more may we expect that the great changes in molecular condition, which apparently trivial causes must bring about on sensitive paper, should elevate or depress its capability of being acted on by light! If I mistake not, it is upon these principles that an explanation is to be given of the successful modes of preparation which Mr. Talbot and Mr. Hunt have described, and the action of the mordants of Sir John Herschel.

630. I therefore infer,

6th. *That the* SENSITIVENESS *of any given preparation depends on its chemical nature and its optical qualities conjointly, and that it is possible to exalt or diminish the sensitiveness of a given compound by changing its optical relations.*

631. 7th. *That, as when radiant heat falls on the surface of an opaque body, the rays reflected are complementary in number to those that are absorbed, so in the case of a sensitive preparation, the rays reflected are complementary in number to those that are absorbed.*

632. This important proposition I prove in the following way: I take a plate, A G, *fig.* 99, three inches by four, and by partially screening its surface, while in the act of iodizing, with a proper piece of flat glass, I produce upon it five transverse bands, *b*, *c*, *d*, *e*, *f*; the fifth, *f*, which has been longest exposed, is of a pale lavender colour; the fourth, a bright blue; the third, a red; the second, a golden yellow; and the first, un-iodized metal; the object of this arrangement being to expose, at the same time and on the same plate, a series of films of different colours and of different thickness, and to examine the action of the rays impinging on them, and the rays reflected by them.

633. Having prepared a second plate, B, and iodized it uniformly to a yellow, I deposited it in the camera, and now placing the first plate, A G, so that the rays coming on it through the window from the sky shall be specularly reflected to the object-glass of the camera, and the image of A G form upon B, I allow the exposure to continue until the yellow of A G is beginning to turn brown; then I shut the camera and mercurialize both plates.

634. In consequence of what has been said (625), it will be readily understood, that of the bands on A G, the first one, which is the bare metal, does not whiten in the mercury vapour; the second, which is yellow, mercurializes powerfully; the third, which is red, is less affected; the fourth, which is blue, still less; and the fifth, which is lavender, hardly perceptibly.

635. But the changes on B, which have been brought about by the rays reflected from A G, are precisely the converse; the band, which is the image of *b*, is mercurialized powerfully; that of *c* is untouched, and absolutely black, *d* faintly stained, *e* whitened, and *f* mercurialized, but little less than *b*.

636. It follows from this, that a white stripe on B corresponds to a black one on A G, and the converse; and for the depth of tint of the intermediate stripes, those of the one are perfectly complementary to the corresponding ones of the other.

637. By the aid of these results, we are now able to give an account of the variability of sensitiveness in photogenic preparations; the yellow iodide of silver is excessively sensitive, because it absorbs all the chemical rays that can disturb it, while the lavender is insensitive, because it reflects them. Under this point of view, sensitiveness, therefore, is directly as absorption, and inversely as reflexion.

638. The superiority of DAGUERRE's preparation over common sensitive paper may now be readily understood. It absorbs all the rays that can affect it, but the chloride of silver, spread upon paper, reflects many of the active rays. The former, when placed in the camera, gives rise to no reflexions that can be injurious; the latter fills it with active light, and stains the proof all over. Hence the Daguerreotype has a sharpness and mathematical accuracy about its lines, and a depth in its shadows, which is unapproachable by the other. Moreover, the translucency of the white chloride of silver, as well as its high reflecting power, permits of particles lying out of the lines of light being affected, the luminous agent being diffused in the paper.

639. The fact, therefore, that a given compound remains unchanged even in the direct rays of the sun, is no proof that light cannot decompose it; it may reflect or transmit the active rays as fast as it receives them. It results from this, that *optical*

forces can control, and even check the play of *chemical affinities.* While thus it appears that there are points of analogy between this chemical agent and radiant heat, we must not too hastily infer that the laws which regulate the one obtain exclusively also with the other. As is well known, there are striking analogies between radiant heat and light, but there are also points of difference; the convertibility of heat of one degree of refrangibility to another does not occur with light; there are also dissimilitudes in the phenomena of radiation and its consequences. I do not doubt that what has been communicated in this memoir will, by the researches of others, be greatly extended; but it is not to be expected that a complete parallel can be run between radiant heat and the chemical rays, any more than between radiant heat and light.

640. From the phenomena of the interference of these rays, of the sensitiveness or non-sensitiveness of the *same* chemical compound being determined merely by the fact of its thickness or thinness, these, and many other similar results, *obviously* depending upon mechanical principles, it seems to me that very powerful evidence may be drawn against the materiality of light, and its entering into chemical union with ponderable atoms. Those philosophers who have endeavoured to prove the undulatory theory, will probably find, in studying these subjects, cogent evidence in favour of their doctrines.

Note added to the preceding Chapter.

ON CERTAIN SPECTRAL APPEARANCES, AND ON THE DISCOVERY OF LATENT LIGHT.

(*Being a Letter to the Editors of the London and Edinburgh Philosophical Magazine, inserted in that Journal, November,* 1842.)

641. Gentlemen—If there be a thing in which I have a disinclination to engage, it is controversy of a personal kind with scientific fellow-labourers. But as, you well know, it ordinarily happens that there is no other gain to philosophers beyond the *mere credit* of their discoveries, they may be forgiven for reluctantly endeavouring to secure this, their only reward.

642. I have recently returned from a long journey, undertaken for the purpose of making trials on the sunlight in lower latitudes, and am surprised to see in the reports that have reached this country of the Proceedings of the British Association, certain announcements received from Professor Bessel, of phantoms which can be produced on surfaces by mercury vapour, by the breath, and other means, *as though the thing were new.* Years ago, if you look in your own Journal (February, 1840, p. 84; Sept., 1840, p. 218; Sept., 1841, p. 198, 199), you will find that I had published facts of the kind; spectral appearances, that could be revived on metals, glass, and other bodies, by the breath, by vapour of camphor, by mercury vapour, &c. The very purpose for which I described them was the striking resemblance of some of them to Daguerreotype images. I have repeatedly shown that, by placing a coin or any other object on iodized silver, *in the dark*, the vapour of mercury will bring out a representation of it. And in one of the papers just quoted, the condition under which camera images

can be reproduced on a silver plate, even after the plate has been rubbed with rottenstone, is described.

643. I have farther seen (Literary Gazette, July 23, 1842, Paris letter), that the fact that light becomes latent in bodies, after the manner of heat, was announced in France as a new and important discovery of Professor MÖSER of Königsburg. In your own Journal, more than a year ago, you printed a long paper written by me on this very topic (September, 1841, p. 196, 204, 205, 206), not merely announcing the fact, but giving rude estimates of the amounts: more exact numerical determinations I have now nearly ready for the press.

644. But I will trouble you no farther with these private matters, simply hoping that your numerous readers, who feel an interest in such things, will turn for themselves to the pages I have quoted.

645. The accompanying photographic impression of the solar spectrum, which I will thank you to give to Sir JOHN HERSCHEL, was obtained in the south of Virginia: probably you can make nothing like it in England: the sunlight here, in New-York, wholly fails to give any such result. It proves that, under a brilliant sun, there is a class of rays commencing precisely at the termination of the blue, and extending beyond the extreme red, which totally and perfectly arrest the action of the light of the sky. This impression was obtained when the thermometer was 96° Fahr. in the shade, and the negative rays seem almost as effective in protecting as the blue rays are in decomposing iodide of silver.

646. The most remarkable part of the phenomenon is, that the same class of rays makes its appearance again beyond the extreme lavender ray. Sir JOHN HERSCHEL has already stated, in the case of bromide of silver, that these negative rays exist low down in the spectrum. This specimen, however, proves that they exist at both ends, and do not at all depend on the refrangibility. It was obtained with yellow iodide of silver, DAGUERRE's preparation, the time of exposure to the sun fifteen minutes.

647. In this impression, six different kinds of action may be distinctly traced by the different effects produced on the mercurial amalgam. These, commencing with the most refrangible rays, may be enumerated as follows: 1st, protecting rays; 2d, rays that whiten; 3d, rays that blacken; 4th, rays that whiten intensely; 5th, rays that whiten very feebly; 6th, protecting rays.

648. It is obvious we could obtain negative photographs by the Daguerreotype process, by absorbing all the rays coming from natural objects, except the red, orange, yellow, and green, allowing, at the same time, diffused daylight to act on the plate.

649. This constitutes a great improvement in the art of photography, because it permits its application, in a negative way, to landscapes. In the original French plan, the most luminous rays are those that have least effect, while the sombre blue and violet rays produce all the action. Pictures produced in that way never can imitate the order of light and shadow in a coloured landscape.

650. If it should prove that the sunlight in tropical regions differs intrinsically from ours, it would be a very interesting physical fact. There are strong reasons to believe it is so. The Chevalier FREDERICHSTAL, who travelled in Central America for the Prus-

sian government, found very long exposures in the camera needful to procure impressions of the ruined monuments of the deserted cities existing there. This was not due to any defect in his lens; it was a French achromatic, and I tried it in this city with him before his departure. The proofs which he obtained, and which he did me the favour to show me on his return, had a very remarkable aspect. More recently, in the same country, other competent travellers have experienced like difficulties, and, as I am informed, failed to get any impressions whatever. Are these difficulties due to the antagonizing action of the negative rays upon the positive.

CHAPTER XIII.

ON A NEW IMPONDERABLE SUBSTANCE, AND ON A CLASS OF CHEMICAL RAYS ANALOGOUS TO THE RAYS OF DARK HEAT.

(From the London, Edinburgh, and Dublin Philosophical Magazine for December, 1842.)

CONTENTS: *Analogies between the Chemical Rays and Heat.—New Nomenclature proposed.—Tithonic Rays.—Independence of Tithonic Rays and Light.—Independence of Tithonic Rays and Heat.—Dark Tithonic Rays.*

651. IN chapter twelve I have pointed out several analogies which may be observed between the phenomena of the chemical rays and those of radiant heat.

652. In this memoir it is my intention to show still more striking points of analogy, and also to direct the attention of chemists to equally striking points of discordance.

653. It will be seen, from the remarkable facts detailed in this paper, that we are now forced to recognise the existence of a new imponderable agent, analogous in many of its properties to light, heat, and electricity, yet differing as much from them all as they do from one another.

654. So far as chemical analogies can direct us, there does not appear anything unphilosophical in the supposition of the existence of many imponderable agents analogous to those already known. The progress of science has indeed tended in different directions in the cases of the imponderable and ponderable bodies. Among the former, we have successively seen the agents that are concerned in galvanic phenomena and those of magnetism merged into electricity; but the ponderable bodies, especially those of a metallic kind, have greatly increased in number, though, so far as their more obvious physical properties are concerned, the differences of many are almost undistinguishable. We have thus found it necessary to invert the maxims of the early cultivators of chemistry, who extended the number of ethereal agents very greatly, and believed that all metals and other ponderable principles were modifications of one or two primordial and elementary forms.

655. Centuries ago it was discovered that the sun's light had the property of effecting chemical changes in bodies, and it is stated that SCHEELE first noticed that this

property was mainly due to the violet rays. Seebeck observed that chloride of silver, exposed to the spectrum, varied its colour with the colour of the space in which it was held, and during the present century a very large amount of new observations has been accumulated. A new art, Photography, has come into existence.

656. The general supposition that obtains is, that the effects in question are due to the rays of light; hence all the words that have been introduced into use have reference to that supposition; such words as photography, photology, photometer, are derived from this erroneous hypothesis, and lead us to confound together things which ought to be kept essentially distinct.

657. As it is the object of this paper, and others which I shall shortly publish, to call the attention of chemists to the agent that is involved in photographic results as a clearly-established and new imponderable substance, possessing striking analogies with light and heat, yet differing as much from them both as they do from each other, I am induced to propose for it a proper name, and to endeavour to establish for it a nomenclature that shall be free from ambiguity, and keep the description of its phenomena separate from those of light. While, therefore, I show that it undergoes radiation, reflexion, refraction, polarization, absorption, interference, &c., under the laws to which its radiant companions, light and heat, are subject, I wish to claim for it a separate and independent existence, to introduce it into the natural family of imponderable agents, with light, heat, and electricity. In that family it stands as the fourth member. Is there any reason that the progress of knowledge should not make known to us multiplied forms of imponderable substances as well as of ponderable matters? This agent differs from light and heat as much as lead differs from zinc or tin.

658. When novel effects, brought about by novel causes, are met with, the purposes of science require new corresponding terms. In the case of the chemical rays of light it is so. I have experienced the need of a nomenclature of the kind from my earliest experiments. It is a rule of which modern philosophers know the value, that such names ought to be free from all attending hypothesis; for if this be not complied with, it soon comes to pass, as knowledge advances, that terms involving theoretical ideas lose much of their significance.

659. The chemical rays are associated with the rays of light, accompanying them in all their movements, originating with them, and, unless disturbed, continuing to exist along with them. But should a compound beam like this fall upon a sensitive surface, the chemical rays sink into it, as it were, and lose all their force, and the rays of light are left alone. Photographic results thus resulting from the reposing of the chemical rays on the sensitive surface are not, however, in themselves durable, as will be shown in this paper, for the rays escape away under some new form.

660. "Tithonus was a beautiful youth whom Aurora fell in love with and married in heaven. The fates had made him immortal; but, unlike his bride, in the course of events he became feeble and decrepit, and, losing all his strength, was rocked to sleep in a cradle. The goddess, pitying his condition, metamorphosed him into a grasshopper."—*Mythological Dictionary.*

661. The fact and the fable agree pretty well; and, indeed, the playful coincidence

might be carried much farther. The powers of photography, which bring architectural remains and the forms of statuary so beautifully and impressively before us, might seem to be prefigured by the speaking image of the son of Tithonus and Aurora, that was to be seen in the deserts of Egypt. And besides this, such words as tithonoscope, tithonometer, tithonography, tithonic effect, diatithonescence, are musical in an English ear. In this paper I shall, therefore, use the term tithonicity and its derivatives in the same manner that we use electricity and its derivatives.

662. This communication takes up the consideration of three distinct facts:

1st. The proof of the physical independence of TITHONICITY and LIGHT.

2d. The proof of the physical independence of TITHONICITY and HEAT.

3d. The proof of the existence of DARK TITHONIC RAYS, analogous to the rays of DARK HEAT. Under this head it will be shown that Tithonicity, like heat, enters transiently into bodies, producing specific changes on them, and then slowly and invisibly RADIATES away. And the physical constitution of the new class of rays thus formed is entirely different from that of rays that come from incandescent sources: a distinction having a striking analogy in the case of heat. Tithonicity becomes *transiently* and *permanently latent* in bodies.

663. Figure 100 serves to show that by the agency of absorbent media we may detect the existence of tithonic rays in every part of the spectrum unaccompanied by light. The results there projected were obtained by an arrangement such as that in *fig.* 101. From a heliostat mirror, *a a*, a beam of the sun's light was thrown in a horizontal position, and falling on a screen, *b b*, a portion of it passed through a circular aperture one fourth of an inch in diameter. At the distance of ten or twelve feet it fell on a glass trough *c c*, with parallel faces, into which any coloured solution could be placed; immediately behind the trough there was a double convex lens, *d d*, of three feet focal length, and between them a second screen, *f f*, with an aperture corresponding to the centre of the lens, half an inch in diameter. Behind the lens was situated a prism of flint glass, *e*, which effected the dispersion of the incident beam. Now the lens not being achromatic, the screen *r v* had to be placed in an inclined position in order to obtain a neat spectrum-image of the hole in *b b*, and this was attended with the great advantage of elongating the total length of the spectrum, and, therefore, increasing the measures. In order to obtain sensitive surfaces of great delicacy, the silver plates were first iodized lightly, and then exposed to the vapour of bromine until they attained a full golden yellow.

664. In *fig.* 100, the line No. 1 represents the *visible* colorific spectrum; it, with No. 2, serves as an index of comparison for all the others. No. 2 represents the effect of a spectrum that has not undergone the action of any absorbent medium on the bromo-iodized plate; the extreme red tinges the plate white, the extreme violet, brown, and all the intermediate space is of a rich brownish violet, with a point of maximum action nearly in its centre. The numerical subdivisions commence with 0 at the extreme red, and are graduated on a principle which I shall explain in a future paper, which makes the spectrum of different tithonographists comparable.

665. No. 3 shows the spectrum after absorption by the persulphocyanide of iron, and

its corresponding tithonograph. This spectrum is divided into three portions, one of which is red and yellow, a second indigo, and a third violet. But the tithonograph exhibits an action far beyond the extreme red, half way through the dark space that intervenes in the middle of the spectrum, both ends of this lower part projecting into dark spaces; while the indigo ray, ordinarily so active, does not tithonize at all.

666. Without going into a long descriptive detail of the comparison of different spectra and their corresponding tithonographs, I shall here sum up the results which may be gathered from an inspection of the plate.

667. By the absorbent action of the persulphocyanide of iron, we can prove the existence of invisible tithonic rays beyond the extreme red—invisible rays corresponding to the green. We can also prove that the indigo-coloured rays of LIGHT may exist without tithonic effect.

668. By the absorbent action of neutral chloride of gold, we can insulate blue-coloured rays of light that are not tithonic.

669. The green solution formed by a mixture of bichromate of potash, muriatic acid, and alcohol, enables us to insulate tithonic rays of the same refrangibility as the violet, but unaccompanied by any light.

670. The solution of sulphate of copper and ammonia enables us to insulate a visible red and yellow ray that are without tithonic power, and an invisible tithonic ray beyond the violet.

671. The solution of litmus enables us to obtain red and green light without action, and an invisible tithonic ray corresponding to the violet.

672. The solution of bichromate of potash enables us to obtain red and orange light without any tithonic effect.

673. Such results might be multiplied without end, for, indeed, there is scarcely an instance in which spectra of rays that have passed absorbent media are exactly coincident with their corresponding tithonographs. To set the matter plainly before the reader, the following tabular view, gathered from the plate, may suffice:

Name of Solution.	Colour of LIGHT without tithonic effect.	Invisible TITHONIC rays corresponding in refrangibility to the
Persulphocyanide of iron . .	Indigo	Extreme red, green.
Chloride of gold	Blue.	
Chrome solution	. . . .	Violet.
Sulph. copper and ammonia .	Red, yellow	Extreme violet.
Litmus	Red, green	Violet.
Bichromate of potash	Red, orange.	

From this, therefore, I infer the entire independence *throughout the spectrum* of the luminous rays that give to the organs of vision the impression of colour and the tithonic rays.

674. When I come to describe the dark tithonic rays that are analogous to the rays of dark heat, and which are unaccompanied by any kind of light whatsoever, no farther doubt can be entertained on this subject. I have also some other proofs of a very remarkable kind, to be described hereafter, drawn from the phenomena exhibited by tithonic rays that have undergone polarization.

675. Next, as to the independence of these rays and the rays of heat.

676. One of the most striking proofs of this is the facility with which impressions of the moon's disc may be obtained on Daguerreotype and other sensitive plates. Even with lenses of comparatively small diameter, and in the space of a few minutes, strong impressions of the moon's surface may be taken. There is no more difficulty in obtaining these sketches than there is in copying a building or a statue, or any other object on which the sun is shining. But the moonbeams have hitherto given no trace of the presence of heat.

677. I found, moreover, by direct trial, that plates which had been carefully prepared, so as to be exceedingly sensitive, were unaffected by the radiant heat of copper at any temperature up to a red heat. These dark rays, therefore, have no kind of effect on such surfaces. A sensitive plate may be made so hot that it cannot be touched, yet its surface remains unchanged; and even the radiant heat emitted by brightly incandescent bodies has no effect, as I also proved.

678. Lastly, *Proof of the existence of* DARK TITHONIC RAYS *analogous to the rays of* DARK HEAT.

679. The experiments now to be described were made with Daguerreotype plates iodized at first to a pale lemon yellow, then brought to a golden hue by immersion in the vapour of bromine, and lastly exposed for a short time to the vapour of iodine again.

680. Having exposed such a plate (*fig.* 102), *a b*, to the action of weak daylight, or lamplight, for a period of time which would cause it to whiten powerfully *all over* if placed in the vapour of mercury, carry it into a room which is totally dark, and suspend at a distance of one eighth of an inch from its surface a metallic screen, *c d*, the under surface of which is blackened. Let all remain in the dark four or five hours, and then remove the sensitive plate *a b*, and expose it to the vapour of mercury. All that portion of it which was not covered by the screen *c d* will undergo no change, but that which was beneath *c d* will whiten powerfully.

681. From this remarkable result I infer that the tithonicity that had originally disturbed the surface of the plate equally all over, has escaped away from those portions that were uncovered, but that its escape has been entirely prevented by the action of the screen; and this must be through RADIATION, for the screen is at a distance, and has never touched the plate. And, farther, that the rays that do thus escape away are absolutely invisible to the eye.

682. Now suppose a piece of black cloth, placed in the rays of the sun until it has become warm, were carried into a cold room, and half its surface screened by some material, as a piece of glass, at a short distance; there cannot be a doubt that the uncovered portion would cool fast by radiation, but the screened portion more slowly, for its radiation would be arrested by the glass plate.

683. The two cases are absolutely alike.

684. Tithonicity, therefore, radiates exactly after the manner of heat.

685. This also furnishes proof, in addition to those I have heretofore given, that not only does tithonicity become LATENT in bodies, but that it becomes latent in two ways, *transiently* and *permanently*, exactly after the manner of heat.

686. The same result is obtained when other sensitive surfaces are employed, the

period of time differing for different bodies. Guided, therefore, by the analogy of heat, I perceive that bodies have a relation to this imponderable agent corresponding to that of *specific heat*. It follows, therefore, with certainty, that,

687. The specific tithonicity of bodies is the prime function on which their sensitiveness depends. Under this point of view the sensitiveness is inversely as the specific tithonicity.

688. The circumstances under which this experiment is made serve also to show that metallic bodies are non-conductors of tithonicity.

689. This contrasts remarkably with their action towards heat.

690. Having exposed a sensitive plate, *a b*, to light until it would whiten if mercurialized, as before, and having prepared a second (*c d*, *fig.* 102) in total darkness, without allowing any light to have access to it, suspend this latter over the former at the distance of one eighth of an inch, so as to cover it about half. Keep the two plates in darkness for several hours, and then mercurialize both. That portion, *a c*, of the first not covered by the second, will not whiten; that portion of the second, *b d*, not covered by the first, will also remain unchanged; but both on those parts that have looked towards each other will whiten.

691. From this I infer, that the portion of the first not overshadowed by the second does not whiten, because its tithonicity escapes away under the form of dark tithonic rays.

692. I also infer, that as both plates are nearly equally whitened on those portions of their surfaces that have looked towards each other, the dark tithonic rays that have escaped from the first plate, notwithstanding their invisibility, have retained their peculiar chemical force, and have affected the second plate.

693. The analogy with heat is here perfectly observed. A hot, *non-conducting* plate, set partially opposite a cold one, would warm that plate on the portion looking towards it, and, through the consequent retardation of radiation, would retain its own heat to a certain extent. But all those portions unopposed by the cold plate would cool down by radiation rapidly.

694. This experiment proves, in a clear and undoubted manner, the total physical independence of tithonicity and light.

695. Hence the absolute necessity of some such nomenclature as that proposed; the chemical rays of light is a misnomer.

696. On the surface of a sensitive plate that has been suitably exposed, as heretofore, place a fragment of perfectly clean and colourless glass. Allow it to remain there for four or five hours in a dark room, then mercurialize, and it will be found that the portion on which the glass has been placed will whiten powerfully, but all the rest will remain unchanged.

697. This, therefore, proves that colourless glass is nearly opaque to the dark tithonic rays, a result observed, also, in the case of the dark rays of heat.

698. I made a comparative trial of the relative permeability of colourless plate-glass and common writing-paper. A sensitive surface was exposed until it had slightly, but very plainly commenced to turn brown. On one portion I now laid a piece of clear

glass, and by the side of it a piece of writing-paper; the arrangement was next placed in the dark for four hours; it was then mercurialized at 160° Fah. for an hour, and the result was very striking. Notwithstanding the long exposure to the mercury vapour, all those portions that had not been covered were perfectly unaffected, the portion that had been covered by the glass was of an intensely deep brown colour, but the portion covered by the paper was marked by a distinct, but very faint white stain. It was therefore plain, that from the uncovered portions all the tithonicity had radiated away; from the portions covered by the writing-paper the same effect almost to the same extent had occurred, the paper, however, slightly obstructing the passage of the rays; but radiation had been wholly prevented from those parts covered by the colourless glass.

699. Writing-paper is, therefore, far more permeable to the dark tithonic rays than the purest plate glass.

700. This property it will be hereafter convenient to speak of under the designation of diatithonescence or transtithonescence.

701. Blue, red, and yellow glass obstruct, to a great extent, the process of radiation. In several trials it seemed as though the yellow was more transparent than the others, but there was not much difference.

702. Transparent rock-salt appears to hold very nearly the same relation of diatithonicity as plate glass.

703. In like manner, the following substances in thin plates obstruct the radiation of tithonicity: Sulphate of lime, beryl, agate, rock-crystal, calc-spar, mica, wafers, metallic bodies, cloth of cotton, wood, ivory, coloured glass, &c., &c.

704. The remarkable results described in the Philosophical Transactions by Sir John Herschel (1840, p. 44), but left by him without any explanation, are of the kind now under discussion. He found that paper washed with nitrate of silver, if exposed to the sun under a piece of glass, darkened much more rapidly than if the glass were away. This effect was by no means limited to that variety of paper, but was observable, also, with many other tithonographic compounds. Transparent minerals, such as topaz, selenite, Iceland spar, quartz, produced the same results as glass. But on gloomy days the phenomena did not appear, a bright sunshine being apparently requisite for their production. " When a piece of nitrated paper, for instance, was rolled round a cylindrical surface of moderate convexity, covered with black velvet, and the piece of glass laid gently in contact with it, the effect of sunshine was exalted at the line of contact; but on either side of that line, as the interval increased, the influence of the glass diminished, and at less than half an inch distance no difference could be perceived between the impressions under the glass and in the free air."

705. Now all this is precisely what should happen if the tithonographic compound radiates while it is undergoing decomposition. The rays which come from the sun pass through the glass with but little loss from absorption; falling upon the nitrate, they decompose it, and now it commences radiating, but the physical character of these rays is very different from the character they possessed before impinging on the nitrate. *Now* they cannot get through the glass, *before* they passed without difficulty. So it is,

precisely, in the case of heat. Much of the heat of the sun passes through plate glass, and if it falls on a dark surface that can absorb it, that surface becomes presently warm and commences radiating; but the physical constitution of these rays is changed: they cannot get through the glass; and if a non-conducting black surface, half covered by a piece of glass and half in the free air, were exposed to the sun, the covered half would, for these obvious reasons, become the hotter. For the same reason, precisely, in the tithonic experiment, the glass increases the final effect by obstructing radiation.

706. It is very obvious why such effects cannot be produced on gloomy days. If at such times we were to expose a piece of black cloth, partially covered by glass, no difference of temperature would be perceptible in its covered and uncovered portions. The reasons are analogous in each case.

707. An experiment, the same in principle as Sir John Herschel's, may be easily made. Upon a sensitive plate that has been exposed a short time to a feeble light, place a convex lens; the arrangement being left for a time in a dark room. When you have mercurialized, you will find a central dark point corresponding with the point of contact, and round it a white areola that shades gradually and imperceptibly away. With a lens with which I have occasionally made this experiment, the areola is nearly an inch in diameter, the lens being a double convex of about two inches focus.

Note added to the preceding Chapter.

ON THE RAPID DETITHONIZING POWER OF CERTAIN GASES AND VAPOURS, AND ON AN INSTANTANEOUS MEANS OF PRODUCING SPECTRAL APPEARANCES.

(*From the London, Edinburgh, and Dublin Philosophical Magazine for March*, 1843.)

708. For some time after I was acquainted with the phenomena mentioned in the last chapter, and there referred to radiation, I was led to attribute them to a peculiar property which certain gases and vapours possess, of which I propose now to give a detailed description.

709. This property is a power of effecting a very rapid detithonization of surfaces that have been *powerfully* tithonized.

710. It affords the means of *instantly* procuring spectral appearances of external forms.

711. Referring now, in the first place, to the analogies of caloric: a body which has been warmed cools down to a temperature that is in equilibrium with that of objects around in several different ways, by radiation, by currents in the air, and often by direct conduction, each of these tending to produce the same result.

712. A sensitive surface, which has been disturbed by exposure to the daylight or lamplight, has the quality of restoring itself to its primitive condition when kept in the dark. Daguerre noticed this in the case of certain resinous bodies; other experimenters have likewise proved that it takes place with some varieties of the ordinary photogenic preparations. I have found that it holds in the coloured films on the surface of silver.

713. Much of this effect is due, as I have endeavoured in the paper above quoted to show, to a direct escape of dark rays by a process analogous to radiation; but much also is due to a hitherto unknown power, possessed by electro-negative gases and vapours, which tends to bring about the same results. So powerfully, indeed, does this cause operate, that, as I have said, for a length of time I attributed all the phenomena to it.

714. I proceed now to describe some simple experiments which will bring this matter clearly before the reader.

715. Take a bromo-iodized silver plate, expose it to the light of the sky or lamplight for a length of time sufficient to brown it sensibly and uniformly all over. In this state, if it were placed in the vapour of mercury, it would solarize or blacken in every part. But, before mercurializing, treat it as follows: Lay upon it a fragment of glass, a piece of metal, or any other object; immerse it for a second or two in a box containing the vapour of iodine; withdraw it, remove the little object, and mercurialize forthwith; and now you will find a perfectly-formed, *black* spectral impression of the object, whatever it was, powerfully brought out by the mercury vapour; but on all those parts to which the iodine vapour has had access the mercury will not adhere, but the phenomenon will take effect as though the plate had never been exposed to the light, except on those portions on which the object, whose spectral image appears, was laid.

716. From this it would seem that the vapour of iodine has the quality of detithonizing a surface that has been changed by light.

717. The same process may be conducted so as to give a still more striking result.

718. Employing a prepared bromo-iodized plate, as before, expose it to any uniform source of light for such a length of time that, if it were mercurialized, it would whiten uniformly, and exhibit the aspect of an ordinary white Daguerreotype. Treat it as before, by placing on it any object, pass it into the vapour of iodine, remove the object, and mercurialize; and now a spectral appearance of that object, of a dense *white* aspect, will emerge, the remainder of the plate being quite black and in the condition of the shadows of a Daguerreotype, that is, as though it had never been exposed to the light.

719. In order to obtain a clear idea of what passes under the foregoing circumstances, I made the following trial.

Upon a plate prepared and deeply tithonized, as has been said, I laid a double convex lens of about two inches focus, and exposing the plate, with the lens upon it, to the vapour of iodine, and then removing the lens, I mercurialized. A deep-blue spectral image emerged, of less diameter than the lens, but, like it, of a circular form, its circumference being marked by as *sharp* a line as if it had been drawn by a pair of compasses. Indeed, it looked as distinct and as *sharp* as if a blue wafer had been laid on the plate.

720. In several successive trials I found that tne magnitude of this spectre diminished as the time of exposure to the vapour had been prolonged.

721. Next, I repeated the same trial, using the plate and lens as just described; but immersing the plate in the vapour of bromine instead of that of iodine, a still

more remarkable image emerged on mercurializing. This image, like the former, was circular and black, but all around it for a certain space there was an annulus of narrow dimensions of pure unmercurialized silver, the deep black of which contrasted strikingly with the blue black of the spectre, and its outer circumference was marked by a faint whitening of the plate—faint, but as sharp as it is possible to conceive.

722. In a third trial, things were conducted as before, except that now chlorine, diluted with atmospheric air, was used; the spectre again came out, and did not differ in any observable manner from that produced by iodine.

723. In a fourth trial, the vapour of nitrous acid was used as a detithonizer. In this case, the edges of the spectre commonly had a gradually shading outline, and only in one instance did I find that sharpness of termination exhibited in the other cases.

724. We therefore perceive that iodine, bromine, chlorine, and nitrous acid can detithonize a surface on which light has fallen: *they can undo what the tithonic rays have done.*

725. In repeating these experiments, as, for example, the one by iodine, if the common iodine-box be used to effect the detithonization, two or three seconds of time is all that is required. A longer period is demanded when the vapour is very weak, but when strong the effect is *almost instantaneous.*

726. This detithonization and production of spectral images can therefore be accomplished in an incredibly short space of time.

727. I made trials with other substances, such as hydrogen gas and the vapour of liquid muriatic acid. The former to a certain extent, though not near so powerfully as the electro-negative bodies mentioned, could produce the change in question; the latter seemed to be without any perceptible action.

728. To the list, with the other electro-negative substances, I believe oxygen ought to be added; for, on repeating the same experiment, and raising the temperature of the plate in atmospheric air so as to maintain the tithonized surface at about 200° Fah. for several minutes, a certain effect, which in an imperfect way resembled those already described, was exhibited. Oxygen, therefore, diluted as in atmospheric air, at 200° Fah., may be regarded as possessing, to a small extent, the property in question.

729. Without multiplying the description of these experiments farther—for the ingenuity of any one who repeats them will suggest many modifications which may give rise to striking results—I will, in conclusion, give the reasons which have led me to suppose that in all these phenomena two different principles are engaged—vapour action and radiation.

730. I have stated that the ELECTRO-NEGATIVE bodies possess this detithonizing quality in a very marked manner. I do not wish it to be understood, however, that there is any relation of antagonization between that particular class of substances and the tithonic rays. It appears to me that their peculiar quality, in the circumstances described, may be traced to the fact that silver, an electro-positive substance, happens to afford the sensitive surface. I have, however, prepared a paper which takes up the consideration of the conditions and theory involved, and will not at present anticipate what has to be offered when that paper shall be published.

731. The action, then, which these different gases and vapours exhibit is so intense as to mask the feebler effect of radiation. Thus, it takes several hours' exposure in the dark, and after a long subsequent process of mercurialization, to prove the true radiant effect, a slow detithonization, which could be brought about by vapour action *in an instant.* But whoever has seen the symmetrical, or, rather, geometrical lines that are left when the slower process is followed, must be struck with the persuasion that the phenomena he witnesses is obeying geometrical laws, and is not due to the irregular action of a dilute and varying current of vapour.

732. Thus, on repeating *carefully* the experiment cited at the close of my last paper, in which a lens is laid on a tithonized surface and left in the dark, I found that after the mercurial process was completed, the plate exhibited a dark central spot surrounded by a white annulus. On drawing upon paper a section of the lens and the sensitive surface, I found that a line drawn from the extreme edge of the white annulus to the edge of the lens was *a tangent* to the lens at that point; that a line drawn from the extreme edge of the central dark spot would, after reflexion by the convex surface of the lens, be found precisely on the edge of the white annulus; the edge of the annulus and the edge of the spot thus having a true catoptrical relation to the curvature of the lens.

733. Now, although in laboratories such as that in which my experiments are conducted, the vapours of these different electro-negative bodies are unquestionably present, and may produce a part of the phenomena witnessed, yet, inasmuch as that phenomenon follows laws that are apparently of a strict geometrical kind, and to those floating vapours we could hardly assign anything like symmetrical results—guided, also, by the analogy of cooling bodies, which lose part of their heat through radiation, and part through the current action of the air, and part through the conducting power of their supports, I have been led to take the view of the phenomena in question which I have set forth.

CHAPTER XIV.

ON A NEW SYSTEM OF INACTIVE TITHONOGRAPHIC SPACES IN THE SOLAR SPECTRUM ANALOGOUS TO THE FIXED LINES OF FRAUNHOFER, AND ON THE TITHONOTYPE.

(*From the London, Edinburgh, and Dublin Philosophical Magazine for May*, 1843.)

CONTENTS: *Mode of producing the fixed Lines.—Description of them.—Difficulty of obtaining them in the Yellow and Green.*
Daguerreotypes are dotted Surfaces.—Mode of copying them by the Tithonotype.—Polarized Structure of the Daguerreotype Film.

734. WHEN a beam of the sun's light, directed horizontally by a heliostat, is thrown into a dark room, and, passing through a chink with parallel sides, is received on the surface of a homogeneous flint-glass prism, which refracts it at the angle of minimum deviation, and, after its passage through the prism, is converged to a focal image on a white screen by the action of an achromatic lens, the spectrum which results is given in great purity, and FRAUNHOFER's lines are quite apparent. The larger ones are seen by the most casual inspection.

735. A tithonographic surface, after being placed in this spectrum, exhibits impressions of an analogous character, being covered with the representations of multitudes of inactive lines varying greatly in dimensions.

736. After several attempts last summer, I succeeded in discovering these lines, and have obtained impressions of them sufficiently perfect.

737. Before proceeding to the description of the mode which is to be followed, and of the characters of the lines themselves, I cannot avoid calling attention to the remarkable circumstance, which has frequently presented itself to me, of a great change in the *relative visibility* of FRAUNHOFER's lines, when seen at different periods. There are times at which the strong lines seen in a red ray are so feeble that the eye can barely catch them, and then, again, they come out as dark as though marked in India ink on the paper. During these changes, the other lines may or may not undergo corresponding variations. The same observation equally applies to the blue and yellow rays. It has seemed to me that the lines in the red are more visible as the sun approaches the horizon, and those at the more refrangible end of the spectrum are obvious in the middle of the day.

738. A beam of the sun, passing horizontally from a heliostat mirror into a dark room, was received on a screen with a slit in its centre, the slit being formed by a pair of parallel knife edges, one of which was movable by a micrometer screw; the instrument being, in fact, the common instrument used for showing diffracted fringes. The screw was adjusted so as to give an aperture $\frac{1}{12}$ inch wide, and the light, passing through, fell upon an equiangular flint-glass prism placed at the distance of eleven feet. Immediately on the posterior face of the prism, the ray was received on an achromatic lens,

the object-glass of a telescope, and brought to a focus at the distance of six feet six inches, at which place an arrangement was adjusted for exposing white paper screens, on which the more prominent fixed lines might be seen, and their position marked, or sensitive plates substituted for the screens, occupying precisely the same position. The lines on the screens could, therefore, be compared with those on the sensitive surfaces, as to position and magnitude, with considerable accuracy. In these trials I have generally used an achromatic lens, but the lines can be beautifully seen by employing a common double convex, if the screen be inclined forward in the way described in (663). Either way answers very well.

739. In order to identify these lines, I have made use of the map of the spectrum, published by Professor Powell, in the Report of the British Association for 1839. With the instrumental arrangement described they are exceedingly distinct, and no difficulty arises in the identification of the more prominent ones. The spectrum with which I have worked occupies upon the screen a space of nearly four inches and a quarter in length from the red to the violet, or, more correctly speaking, from the ray marked in that map A to the one marked *k*. In stating, however, that no difficulty arises in identifying these lines, I ought to add that I am referring to that particular map. In the figure annexed to Sir John Herschel's Treatise on Light, in the *Encyclopedia Metropolitana*, the ray marked G seems to differ from that of the report. But Professor Powell's map being drawn from his personal observations, and with reference to these very difficulties, as it coincides with my own observations and measures, I have employed it, and, therefore, take the letters he gives.

740. It will be understood that the *whole* spectrum and *all* its lines cannot be obtained at one impression. The difficulty which is in the way of effecting this rests in the circumstance that different regions of the spectrum act with different power in producing the proper effect. Thus, if on common yellow iodide of silver the attempt were made to procure all the lines at one trial, it would be found that the blue region would have passed to a state of high solarization, and all its fine lines become extinguished by being overdone long before any well-marked action could be traced at the less refrangible extremity. We have, therefore, to examine the different regions in succession, exposing the sensitive surface to each for a suitable length of time.

741. In *fig.* 103, I have given on the left side a representation of the larger lines of Fraunhofer, the letters being derived, as has been said, from Professor Powell's map. The position of the lines is, however, copied from my own spectrum as closely as I have been able to accomplish it.

742. In order that a comparison may be made between the new system of lines and those of Fraunhofer, the right side of the plate gives a tithonographic representation of them as obtained on a Daguerreotype plate which has been iodized to the yellow, brought by the vapour of bromine to the red, and then slightly exposed to the vapour of chloride of iodine. The map is so adjusted in the plate as to have its lines by the side of those of Fraunhofer which have the same name. Referring, therefore, to the plate, it will be seen that there are, beyond the red ray, three extra-spectral lines, which I have marked α, β, γ. These, however, I have only occasionally found, for, from the general

diminution of effect in that region, they do not always come out in a plain and striking manner. None of FRAUNHOFER's lines in the yellow and green are given, but G and its companions are very strongly marked, as also the group about *i*. But by far the most striking in the whole tithonograph are those marked H and *k*; and now, passing beyond the violet, and out of the visible limits of the spectrum, four very striking groups make their appearance. The first line of each of these groups I have marked, in continuation of FRAUNHOFER's nomenclature, M, N, O, P. In L there are three lines, in M five, in N three, in O three, and in P five.

743. Besides these larger groups, the whole tithonograph is crossed by hundreds of minuter ones, so that it is utterly impossible to count them. If, as it has been said, nearly 600 have been counted between A and H, I should think there must be quite as many between H and P. In speaking, therefore, of these lines as though they were strong individual ones, the expression is to be taken with some limitation. It is quite likely that each of those bolder lines is made up of a great number that are excessively narrow and close together.

744. If the absorptive action of the sun's atmosphere be the cause of this phenomenon, that action takes place much more powerfully on the more refrangible and extra-spectral region. The lines exhibited there are bold and strongly developed; they are crowded in groups together.

745. I cannot doubt, judging from analogy, that, by proper modes of investigation, similar lines might be detected in the calorific extra-spectral region.

746. The contrast between the visible and tithonographic spectra is maintained by the non-appearance of lines in the yellow and green regions. Once only I thought I perceived a line corresponding to FRAUNHOFER's F, but it was exceedingly faint, and, on the whole, doubtful.

747. FRAUNHOFER's lines, which occur on the orange, yellow, and green spaces, thus leaving no corresponding impression, another argument is furnished of the independence of the tithonic and luminous rays. It is probable that more perfect arrangements than I have used would give the *whole* spectrum as though it were full of these inactive spaces; and in stating that nothing like FRAUNHOFER's lines exist in those medial regions, I therefore simply wish it to be understood that I can find nothing at all corresponding in magnitude to the great lines marked D, E, F, though hundreds of microscopic ones may probably exist in these very spaces.

748. The position of the lines as represented on the sensitive surface is found to be, as might have been anticipated, independent of the chemical nature of that surface. The iodide of silver gives them in the same places as the bromide.

749. An argument might be drawn, as has been said, from the absence of these lines in the yellow and green spaces, as to the independence of the dark rays and light. This is, however, only another proof of a fact of which we have now abundant evidence. In 1834, when my attention was first fixed forcibly on these things, and I began to make prismatic analyses by the aid of sensitive paper, some of my first trials were directed to the detection of these fixed lines. At that time I was employing sensitive paper made with the bromide of silver, precisely as has been subsequently done in Eu-

rope; a number of the results were published in the American Journals during the year 1837. In the detection of these lines I failed entirely; but the bromuretted paper enabled me, at that early period (while the attention of no other chemist was as yet turned to these matters), to trace the blackening action from far beyond the confines of the violet down almost to the other end of the spectrum. I distinctly made out that the dark rays underwent interference after the manner of their luminous companions, a result originally due to ARAGO, and printed some long papers in proof of the physical independence of the chemical rays, and light, and heat, throughout the spectrum.

750. In a paper "On the Action of the Rays of the Solar Spectrum on the Daguerreotype Plate," inserted in the Philosophical Magazine for the month of February, 1843, which has just reached me, Sir JOHN HERSCHEL points out that a connexion may be traced between the phenomena of coloration impressed by the spectrum and those of NEWTON'S rings. With striking ingenuity, he shows how a succession of positive and negative pictures may arise by prolonged solar action, and those shades of colour which the iodide of silver exhibits, under variable exposure to light, originate.

751. This hypothesis, however, as that able philosopher proceeds to state, is not unattended with difficulties, and after pointing out what those difficulties are, he shows how, nevertheless, it can account for an extensive group of facts. I regret that these difficulties are in the way, and that there are also other facts which appear to exclude the theory of thin plates from these phenomena.

752. *The Daguerreotype image, in all its forms, may be transferred by any copying process to other suitable surfaces. In other words, it may be printed from.*

753. Sir DAVID BREWSTER was the first to show that the colours of mother-of-pearl might be impressed on any yielding surface. In the same manner so can the Daguerreotype image.

754. This is, unquestionably, the most important fact yet known in the history of these mysterious images, both in a theoretical and in a practical point of view. In a theoretical point of view, it shows us that it is among the phenomena of grooved, or striated, or dotted surfaces that the Daguerreotype is to be ranged; and, in a practical point of view, it shows the true mode of solving the great problem of producing from a given proof a multitude of copies.

755. In (594), in speaking of the action of isinglass dried on the surface of the Daguerreotype pictures, I stated that I had succeeded with a process for multiplying copies, and promised on a future occasion to make it known: that promise I now proceed to redeem.

756. On referring to the paper in question, the reader will perceive that the following facts are stated (577): that gum-arabic mucilage, dried on a common Daguerreotype, splits up, bringing with it the white portions; that Russian isinglass, (584) and (591), dried in a similar manner, does the same thing, and will even rend off the yellow coating of iodine if it has not be n previously removed.

757. Now, in addition, I have to state that, if on a picture that has been fixed by a film of gold, so as to be irremovable, a layer of isinglass be caused to dry and split up,

it will bear on its surface a complete impression of the drawing, all the details being given with inexpressible beauty, the minutest lines and dots being present.

758. From the same plate a series of these impressions may be taken. The images that are on them may be seen either by reflected or transmitted light; in the former instance, most favourably by placing them on black velvet.

759. I have hopes of improving this method so as to introduce it into effectual use. The practical difficulties that are in the way rest in the circumstance that the isinglass often spilts off in chips instead of separating in one unbroken sheet. And the plate from which the impressions are taken, or with which the printing process is carrying on, becomes injured; not by having its surface removed, but by the isinglass adhering in circumscribed places, and obstinately refusing to detach itself.

760. This refinement on the art of printing, or, rather, of casting, might be supposed to give rise to very perishable results. This, however, is far from the case; I have now by me proofs made nearly two years ago, and they do not seem to have undergone any change. They have lain loosely in a drawer.

761. I presume, therefore, that any process which can exhibit the colours of mother-of-pearl will also exhibit Daguerreotype images. This lays open a variety of new branches of the photographic art.

762. As a name for these processes of copying the surface of a Daguerreotype, I would suggest the word TITHONOTYPE.

763. To carry this process into effect, the operator proceeds as follows: The Daguerreotype which he designs to copy is to be covered with a thin film of gold in the usual way, care being taken that the film is neither too thick nor too thin. If it be too thick, the resulting copy is injured, and difficulties are more liable to arise in effecting the separation of the gelatinous coat; if too thin, the plate itself will suffer injury by having the figure torn off.

764. A clear solution of isinglass is next to be prepared; it must be of such a consistency that a drop of it poured on a cold metallic plate will speedily set. Much of the success of the process depends on this solution being properly made. There is a substance in the market which goes under the name of Cooper's isinglass, which I have found much better than any other for these purposes.

765. The plate is to be arranged horizontally, with its face upward, on some proper support, in the current of hot air that rises from a stove. The isinglass is to be poured on until a stratum about $\frac{1}{8}$ of an inch deep is upon the plate. It is then suffered to dry, the process being conducted so as to occupy two or three hours. When perfectly successful, as soon as the drying is complete, the film of isinglass, now indurated into a tithonotype, splits off, and on being examined either by reflected or transmitted light, will be found to bear a minute copy of the original.

766. To return for a while to the theory of these images. While thus it is plain that the optical effect depends on surface configuration alone, and does not seem to have any immediate relation to the thickness or thinness of a film, it is very different with the chemical effect on which the whole phenomenon depends.

767. *The Daguerreotype film, which has been under the influence of light, is polarized throughout its structure previous to mercurialization.*

768. I use the word "polarized" in its chemical sense. An illustration will serve to show the signification I attach to the term. When water is placed between platina electrodes, its oxygen is liberated from one of them, and its hydrogen from the other, and the intervening liquid assumes a polar state, a series of decompositions and combinations going on. As that water is polarized, and undergoes polar decomposition, so, too, do the same phenomena hold in the case of the Daguerreotype film.

1st. We know that no iodine is ever evolved from the plate, even under the most prolonged action of the light (587).

2d. The cause of the final appearance of the image is due to silver being liberated on the anterior face of the plate (592).

3d. When, by the action of gelatine, the iodine and mercury are both removed from the plate, it is obvious that the plate has been corroded wherever the light fell. Iodine, therefore, has been evolved on the posterior face of the film, and is the cause of this corrosion.

769. From the circumstance, therefore, that iodine is evolved at the back of the film and silver at its front, and the film itself remaining the same in thickness throughout, it is obvious that there is a strong resemblance between this phenomena and that of the polar decomposition of water. The electro-positive and electro-negative elements are yielded up on opposite faces of the film, and its interior undergoes incessant polar changes, the opposite electric particles sliding, as it were, on one another.

770. In a note to the preceding chapter (708, &c.) I have described the remarkable power of certain electro-negative gases in producing the rapid detithonization of surfaces that have been changed by light. Since that paper was sent to England, I perceive, from the "Scientific Memoirs," that Professor Moser has published results of a similar kind. The true explanation of them appears to me to be very different from that which he gives; for his idea of vapours containing latent rays of particular orders of refrangibility or colour, rests on a very feeble analogy, and strikes me as entirely without support.

771. The view which I have taken of these phenomena, and to which allusion was made in the paper referred to, can be easily understood from what has just been said. The film on a Daguerreotype plate, which has been disturbed by the tithonic rays, but not yet mercurialized, is in a *polar condition of force*, its iodine is ready to unite with a new layer of silver behind, its silver is ready to be evolved in front. If it be exposed to mercurial vapours, union at once takes place on that front face, and an amalgam is formed; if to the vapours of iodine, or chlorine, or bromine, an iodide, chloride, or bromide of silver is formed. In an instant, its disturbing affinities being satisfied, the film reverts back to its former condition of equilibrium, and is precisely in the condition it was in before exposure to the light.

CHAPTER XV.

ON THE DECOMPOSITION OF CARBONIC ACID GAS AND THE ALKALINE CARBONATES BY THE LIGHT OF THE SUN, AND ON THE TITHONOTYPE.

(From the London, Edinburgh, and Dublin Philosophical Magazine for September, 1843.)

CONTENTS: *Dr. Daubeny's Experiments.—Importance of the Subject.—Decomposition in the Prismatic Spectrum.—Decomposition under Absorbent Media.—Decomposition is due to Light.—Disturbing Causes.—Analysis of Gas evolved.—Decomposition of Saline Bodies.—Production of Nitrogen.—Disappearance of Oxygen.—Character of Chlorophyll.*
Tithonotypes in Copper.—Detithonizing Power of Gases.

772. FOR many years it has been known that the green parts of plants under the influence of the sunlight possess the power of decomposing carbonic acid, and setting free its oxygen. It is remarkable that this, which is a fundamental fact in vegetable physiology, should not have been investigated in an accurate manner. The statements met with in the books are often far from being correct. It is sometimes said that pure oxygen gas is evolved, that the decomposition is brought about by the so-called "chemical rays;" these, and a multitude of other such errors, pass current. So far as my reading goes, no one has yet attempted an analysis of the phenomenon by the aid of the prism, the only way in which it can be truly discussed.

773. In a paper by Dr. DAUBENY, inserted in the Philosophical Transactions for 1836, two facts, which I shall verify in this communication, are fully established. These are, 1st, the constant occurrence of nitrogen gas in mixture with the oxygen, an observation originally due to SAUSSURE, or some earlier writer; and, 2d, that the act of decomposition is due to the LIGHT of the sun. This latter result, obtained by employing coloured glasses or absorbent media, has not been generally received. Doubt will always hang about results obtained in this way, and nothing but an analysis by the prism can be satisfactory. It has happened, therefore, in books of credit published since that time, that other interpretations of the phenomena have been given.—*Johnson's Agr. Chem.*, *Lect.* v., § 7; *Graham's Chem.*, p. 1013.

774. In its connexions with modern organic chemistry and physiology, the experiment of the decomposition of carbonic acid by leaves assumes extraordinary interest. To no other single experiment can the same importance be attached. When we remember that this decomposition is the starting-point for organization out of dead matter, that, commencing with this action of the leaf, the series of organized atoms goes forward in increasing complexity, and blood, and flesh, and cerebral matter are at its terminus, it is clear that unusual importance belongs to precise views of this, the commencing change. The beams of the sun are the authors of all organization.

775. There is but one way by which the question can be finally settled, and that is by conducting the experiment in the prismatic spectrum itself. When we consider the

feebleness of effect which takes place by reason of the dispersion of the incident beam through the action of the prism, and the great loss of light through reflection from its surface, it might appear a difficult operation to effect a determination in this way. Encouraged, however, by the purity of the skies in America, I made the trial, and have met with complete success.

776. Before entering on the experiments which I have to communicate, I cannot avoid once more impressively calling the attention of chemists to the true character of those emanations which are here designated "tithonic rays." It is not enough that we admit the existence, throughout the spectrum, of dark rays, possessing the power of bringing about chemical changes; it is not enough that we call them chemical rays; there are qualities of distinction appertaining to them which mark them out as being specific in their kind, properties which they possess totally distinct from those of light and heat. Their title to the rank of a distinct imponderable agent is just as perfect as that of light or heat. From heat they are to be distinguished by incapacity for metallic conduction, and by want of the power of expanding bodies; from light, by failing to give any impression to the organ of vision. According to the recognised rules of chemistry, they ought to be received as a fourth imponderable agent.

777. It is not sufficient, as has been said, to call them "chemical rays." The term implies that the distinctive characteristic pertaining to them is the power of changing the composition of bodies. But do not the rays of heat eminently produce like changes? Are not half the decompositions in chemistry brought about by the action of caloric? As respects LIGHT, many instances are already known in which it produces decompositions and combinations; as will be presently shown, it is the agent that brings about the decomposition of carbonic acid. The faculty of producing a like effect is not the distinguishing quality of the tithonic rays, nor can the term *chemical* be any more applied to them than to either of their acknowledged distinct companions. *Unless, therefore, chemists are content to admit that a species of heat may exist devoid of the power of expanding bodies, of giving the sensation of warmth, and of being transmitted by conducting processes; or, unless they admit that light can exist in such a modified condition as to produce in our eyes the sensation of darkness, they will have to admit these tithonic rays as constituting a fourth imponderable agent.* The name they may take is not a matter of importance; that which is least trammelled by hypothesis is best. It is not the object of this and the foregoing chapters to show merely that a class of invisible rays exists in the spectrum; that has been known for a long time; but it is to point out the true relation of these rays to other bodies and other forces in the world, to assert for them their title of a fourth distinct imponderable agent, and to secure for them the admission of that title by giving them a name.

778. When the leaves of plants are placed in water from which all air has been expelled by boiling, and exposed to the sun's rays, no gas whatever is evolved from them. When they are placed in common spring or pump water, bubbles quickly form, which, when collected and analyzed, prove to be a mixture of oxygen and nitrogen gases; from a given quantity of water a fixed quantity of air is produced. When they are exposed in water which has been boiled and then impregnated with carbonic acid, the decomposition goes on with rapidity, and large quantities of gas are evolved.

779. The obvious inference which seems to arise from these facts is, that all the oxygen collected is derived from the direct decomposition of carbonic acid. We shall presently examine whether this is the correct inference.

780. Having, by long boiling and subsequent cooling, obtained water free from dissolved air, I saturated it with carbonic acid gas. Some grass leaves, the surfaces of which were carefully freed from any adhering bubbles or films of air by having been kept beneath carbonated water for three or four days, were provided. Seven glass tubes, each half an inch in diameter and six inches long, were filled with carbonated water, and into the upper part of each the same number of blades of grass were placed, care being taken to have all as near as could be alike. The tubes were inserted side by side in a small pneumatic trough of porcelain. It is to be particularly remarked that the blades were of a pure green aspect, as seen in the water; no glistening air-film, such as is always on freshly-gathered leaves, nor any air bubbles, were attached to them. Great care was taken to secure this perfect freedom from air at the outset of the experiments.

781. The little trough was now placed in such a position that a solar spectrum, kept motionless by a heliostat and dispersed by a flint-glass prism in a horizontal direction, fell upon the tubes. By bringing the trough nearer to the prism or moving it farther off, the different coloured spaces could be made to fall at pleasure on the inverted tubes. The beam of light was about three fourths of an inch in diameter. In a few minutes after the commencement of the experiment, the tubes on which the orange, yellow, and green light fell, commenced giving off minute gas bubbles, and in about an hour and a half a quantity was collected sufficient for accurate measurement.

782. The gas, thus collected in each tube, having been transferred to another vessel and its quantity determined, the little trough, with all its tubes, was freely exposed to the sunshine. All the tubes now commenced actively evolving gas, which, when collected and measured, served to show the capacity of each tube for carrying on the process. If the leaves in one were more sluggish, or exposed a smaller surface than the others, the quantity of gas evolved in that tube was correspondingly less. As may be readily supposed, I never could get tubes so arranged as to act *precisely* alike, but after a little practice I brought them sufficiently near to equality. And in no instance was this testing process of the power of each tube for evolving gas omitted after the experiment in the spectrum was over.

TABLE OF THE DECOMPOSITION OF CARBONIC ACID BY LIGHT OF DIFFERENT COLOURS.

Experiment 1.		Experiment 2.	
Name of Ray.	Volume of Gas.	Name of Ray.	Volume of Gas.
Extreme red . .	·33	Extreme R. and red	·00
Red and orange .	20·00	Red and orange . .	24 75
Yellow and green	36·00	Yellow and green .	43·75
Green and blue .	·10	Green and blue . .	4·10
Blue	·00	Blue	1·00
Indigo	·00	Indigo	·00
Violet	·00	Violet	·00

783. From this, it appears that the rays which cause the decomposition of carbonic acid gas have the same place in the spectrum as the orange, the yellow, and the green; the extreme red, the blue, the indigo, and the violet exerting no perceptible ef-

fect. This being the case, we should expect that, by passing a beam through absorbent media of such a nature that the extreme red, the blue, the indigo, and violet are absorbed, this decomposition should nevertheless go on. A solution of bichromate of potash nearly fulfils these conditions, and not only does it absorb the luminous rays in question, but also all the tithonic rays, except a trace of those which correspond to the more refrangible yellow and less refrangible green.

784. A remarkable proof of the correctness of the foregoing prismatic analysis comes out when leaves are made to act on carbonated water in light which has passed through a solution of bichromate of potash. I took a wooden box of about a cubic foot in dimensions, and having removed its bottom, adjusted to it a trough made of pieces of plate glass. The box being set on end, its lid served as a door, and the trough being filled with a solution of the bichromate of potash, the sun's beams came through it, and in the interior of the box an arrangement of leaves and carbonated water could be exposed to the rays that had escaped absorption. The thickness of the liquid stratum was about half an inch. I had several such boxes made, so that I might compare the simultaneous effect of light which had undergone absorption by different media. They formed, as it were, a series of little closets in which bodies could be exposed to party-coloured light—blue, yellow, red, &c.

785. Whenever an experiment was commenced in these closets, simultaneously a similar one was commenced in the unobstructed sunshine. It is needless to repeat, that in all these care was taken to have the different arrangements for decomposition as nearly alike as possible.

786. On comparing together the amount of gas evolved in unabsorbed light and in light that had undergone absorption by the bichromate of potash, in three out of five trials the gas collected under the latter circumstances exceeded in volume that collected under the former; this was probably due to a slightly higher temperature which obtained in the box.

787. On comparing together the volumes of gas collected under the bichromate of potash and under litmus water, the latter was not equal to one half the former.

788. I compared together the gas evolved in unobstructed light, under bichromate of potash, and under ammonio-sulphate of copper; the results were as follows:

Unobstructed light	4·75
Bichromate of potash . . .	4·25
Ammonio-sulphate of copper .	·75

789. Comparing these experiments, made by the aid of absorptive media, with those made by the prism, we are enabled to come to a definite conclusion as to the character of the rays which cause this decomposition.

790. The true office of prismatic analysis is to determine the refrangibility of the rays which produce given actions; but inasmuch as rays of heat, rays of light, and tithonic rays are found throughout the spectrum, in many cases the prism fails to indicate to which of these imponderable agents phenomena are to be ascribed. The case before us furnishes a striking example. Although the decomposition of carbonic acid is most energetically brought about by rays whose index of refraction corresponds to

the yellow, yet that region of the spectrum is far from being devoid of heat and tithonicity.

791. By considering, however, the prismatic analysis and the absorptive analysis together, the following facts appear: 1st, the place of maximum action in the spectrum corresponds to the maximum of illumination; 2d, at the place of the maximum of heat (which in the prism here used is beyond the extreme red) no decomposition whatever takes effect; this appears, therefore, to exclude calorific influence; 3d, the point of maximum action of the tithonic rays, which escape absorption by the bichromate of potash, being towards the green, does not correspond with the place of maximum decomposition, which is the yellow; this seems to exclude the tithonic rays; 4th, the decomposition taking place almost as energetically under the bichromate of potash as in the unobstructed beams of the sun, and that salt absorbing all but a mere trace of the tithonic rays, if the effect was due to them it ought to be retarded to an extent corresponding to their loss by absorption, which is far from being the case; the retardation which is observed appearing to be attributable rather to the loss of light by reflexion from the faces of the trough, and the partial turbidity (want of translucence) of its glasses and solutions.

792. For these reasons, I conclude that the decomposition of carbonic acid by the leaves of plants is brought about by the rays of LIGHT; and that the calorific and tithonic rays do not participate in the phenomenon. As was stated before, therefore, the rays of light are just as much entitled to the appellation of chemical rays as those which have heretofore passed under that name.

793. I might observe, in passing, that there is a degree of precision attached to results of the decomposition of carbonic acid which is wholly wanting in most similar experiments. In the stains on Daguerreotype plates, or on photographic papers, though there is no difficulty in ascertaining the place of maximum effect, yet nothing in the shape of absolute measures of quantities can be obtained. When, however, gas can be collected and its volume determined, as in the voltameter and in the experiments just described, the results possess a degree of exactness which enables us to draw from them definite conclusions.

794. Let us now proceed to determine the constitution of the gaseous mixture given off during these decompositions. It is not pure oxygen, as has often been supposed and often disproved, but a mixture of oxygen, nitrogen, and carbonic acid. It is mainly to the ratio of the two former that attention has to be directed; the amount of the latter is always variable in different trials. Before proceeding to this, there are certain observations to be premised, the results of which, though familiar to chemists accustomed to gaseous analysis, deserve a place here, for they seem to be wholly overlooked in many of the experiments connected with the so-called respiration, but, rather, digestion of plants recorded in the books of botany.

795. When gas of any kind is confined over water in the pneumatic trough, its constitution is undergoing incessant change. A portion of it dissolves more or less slowly in the water, and in exchange it receives from the water gas, which is always dissolved therein. If two jars, filled with different gases, stand side by side on the shelf, each

is incessantly disturbing the constitution of the other, nor does this disturbance cease until the contents of both jars are chemically the same. There are some beautiful experiments of easy repetition which serve to show how rapidly gases and vapours can thus percolate through fluids. Take a pint bottle, and pass through its cork, which ought to fit it very loosely, a glass tube a foot long, drawn narrow at its upper end. Into the bottle put a few drops of water of ammonia. Dip the wide end of the tube into a solution of soap, and introduce it into the interior of the bottle, adjusting it in such a position by the cork, that when air is blown in at the narrow end, the soap-bubble which expands at the wide end may occupy the middle of the bottle. Placing the lips on the narrow end, blow a bubble an inch or more in diameter, and, without loss of time, cautiously draw back again the air from the interior of the bubble into the mouth. A strong ammoniacal taste is at once perceived. Now it is obvious that this ammonia must have passed with very great rapidity through the bubble.

796. A still more instructive experiment may be easily made. Take a three-ounce bottle, with a wide neck, close the mouth of it by a film of soap-water, by passing the moistened finger over it. Place it under a jar of protoxide of nitrogen. Instantly the horizontality of the film is disturbed; it swells upward, and is spontaneously expanded by the passage of the gas through it into a bubble. The play of colour which attends this experiment, and the excessive thinness which the film finally assumes, render this one of the most beautiful experiments that chemistry can furnish; for when the bubble is almost invisible by reason of its incapacity to reflect light, and can only be seen in particular positions, it still discharges its percolating function.

797. This percolation of gases through liquids cannot be hindered by employing oil, or such other liquids as botanical writers seem to imagine. Through common lamp-oil, through copaiva balsam, &c., hydrogen gas will escape with rapidity, and protoxide of nitrogen and carbonic acid still faster. The law that regulates these phenomena is a very simple one: the gas escapes through the confining medium with a rapidity proportioned to its solubility therein.

798. These things being understood, it is obvious that when carbonic acid is decomposed in the experiments we have been detailing, a variable portion of that gas will intermingle with the oxygen collected. The proportions must be variable, for it depends on the amount of carbonic acid remaining behind in the water, on the speed with which the experiment is conducted, and other variable conditions. As before stated, therefore, I shall leave out of consideration this carbonic acid, in discussing the analysis of the collected gases, because it is present by accident, and is not essentially connected with the phenomena, except in one instance, where dark heat is to be employed, as will be described presently.

ANALYSIS OF AIR EVOLVED FROM CARBONATED WATER BY THE SUN.

Exp.	Name of Plant.	Oxygen.	Nitrogen.
1.	Pinus tæda . . .	16·16	8·34
2.	do. . . .	27·16	13·84
3.	do. . . .	22·33	21·67
4.	Poa annua . . .	90·00	10·00
5.	do. . . .	77·90	22·10

799. I may remark, that this table contains a few out of a great number of experi-

ments, all of which might have been quoted as examples of the observations which I wish to deduce from it. 1st. They all coincide in this respect, that the oxygen is never evolved without the simultaneous appearance of nitrogen. 2d. That when certain leaves are employed, as those of the *Pinus tæda*, there seems to be a very simple relation between the volumes of oxygen and nitrogen. In the first and second of those experiments, the volume of the oxygen is to that of the nitrogen as two to one; in the third, as one to one. In certain cases this apparent simplicity of proportion is departed from; but from its frequent occurrence in many analyses I have made, it seems to demand attentive consideration. Moreover, in other plants, as in experiments 4 and 5, the amount of oxygen is relatively greater, and between it and the nitrogen there does not appear any exact proportion.

800. In order to ascertain whether decompositions taking place under absorbent media, as bichromate of potash, produce the same results as indicated in the foregoing table, I made several analyses of gas collected under these circumstances. The presence of the absorbent medium did not seem to exert any influence whatever, the general results coming out as though it had not been employed.

801. It has long been a matter of popular observation that the sunlight has the quality of extinguishing domestic fires. I do not know whether there is in reality any ground for this opinion; or if so, whether the phenomenon is in any way connected with the relations of light to carbon and oxygen. Popular opinion ascribes the effect to the light, and not the heat of the ray. To determine whether radiant heat, unaccompanied by light, had the power of producing the decomposition of carbonic acid through the agency of leaves, I placed in the focus of a large brass concave mirror a vessel containing some pine leaves in carbonated water. The mirror was set before a wood fire, and after a little time the leaves began evolving bubbles. The temperature of the water rose as high as 140° Fah., and when sufficient gas was collected, examination proved that nearly the whole of it was absorbed by lime or potash water. From this, it is evident that radiant heat merely liberates the carbonic acid, and does not decompose it. This corroborates, therefore, the result of the prismatic analysis, that it is the light, and not the heat, which brings about the change.

802. *Decomposition of Alkaline Salts.*—The conditions under which carbonic acid gas is decomposed being understood, I pass now to the description of similar decompositions occurring in the case of saline bodies. It has always been a subject of surprise to chemists, that the powerful affinity by which carbon and oxygen are held together should be so easily overcome at common temperatures. Even potassium cannot decompose carbonic acid in the cold. It might, therefore, be reasonably expected that the energetic forces which bring about this change ought also to effect other remarkable decompositions. In fact, as I shall now proceed to show, the decomposition of carbonic acid is only one of a very numerous series.

803. The alkaline bicarbonates, as is well known, undergo decomposition by a slight elevation of temperature. When boiled with water they gradually give off their second atom of acid, and slowly pass into the condition of neutral carbonate. This easy decomposibility led me to inquire whether green leaves, under the action of the sunlight,

would effect the liberation and subsequent reduction of the acid. In the following experiments it is to be observed, that the boiling is not continued long enough to affect to any extent the constitution of the salt, and in each case any portion of free carbonic acid extricated during the cooling of the liquid was removed by the action of the air-pump. The solution, when finally used, contained no gaseous matter, but only the salt dissolved in water.

804. Having boiled some distilled water to expel all gaseous matter, dissolve in it a small quantity of bicarbonate of soda. Introduce into a test tube some leaves of grass, fill the tube with the saline solution, which has been once more boiled to expel any air it may have obtained from the dissolving salt, and invert the tube in some of the solution in a wine-glass, after having carefully removed all adhering bubbles of air from the leaves by a piece of wire, or in any other convenient manner. This arrangement, kept in the dark, undergoes no change; but, if brought into the sunshine, bubbles of gas are rapidly evolved, and in the course of a few hours the tube becomes half full. On detonation with hydrogen this gas proves to be rich in oxygen.

805. I made some attempts to discover how much oxygen could in this way be evolved from known quantities of bicarbonate of soda, supposing it probable that the second atom of carbonic acid being removed and decomposed, the process would cease. I need not detail the result of those trials; they indicated that the supposition I had formed was not correct. The process is not limited to the removal and decomposition of the second atom, but goes forward, the first atom itself being in like manner decomposed. From this it would seem that carbonate of soda itself should be decomposed, and experiment verifies the conclusion; for, on using that salt instead of the bicarbonate, the evolution of oxygen goes on precisely in the same way.

806. As in these experiments *solid* salt dissolved in water is decomposed, it is obvious that the function by which the leaves accomplish this is very different from that of respiration. It is not respiration, but a true digestion.

807. Liebig has shown that ammonia exists in the ascending sap. It is probable, therefore, that it does not undergo final change before reaching the upper face (sky-face) of the leaf. There, if it be in the form of a carbonate, it unquestionably is concerned in decomposition. With the natural experiment before us, we might expect that the carbonate of ammonia used in place of the soda salts of the last experiment would yield like them. Accordingly, it will be found, by using the officinal sesquicarbonate of ammonia, that leaves effect its decomposition. In numerous experiments it has yielded me gas frequently containing more than 90 per cent. of oxygen.

808. In every instance which I have examined, the gas evolved from leaves is not pure oxygen, but, as has been said, a variable mixture of oxygen and nitrogen. This result is of uniform occurrence; I have observed it in low latitudes, where the sun is extremely brilliant, in the case of different plants; and on referring to Dr. Daubeny's paper ,it will appear that he has uniformly recognised the same result in England. The very remarkable qualities which certain nitrogenized substances are known to exhibit, acting as ferments as they are undergoing decay, might lead to the suspicion that the decomposition of carbonic acid by leaves is due to the action of some nitrogenized body, the eremacausis of which is promoted by the rays of the sun.

809. There are many facts which go to prove that the decomposition of carbonic acid is a secondary result, brought about by the action of nitrogenized ferment in a state of eremacausis, the sunlight operating in the first instance upon the ferment itself. Plants can grow in a certain manner in dark places, and if the observations of botanists have been correctly made, although this kind of growth may be abnormal, it eventuates in increasing the total weight of carbon. It signifies little that in these instances lignin may often be deficient, for other bodies of the starch family make their appearance; and results of this kind serve to show that, though in all ordinary cases the union of carbon with the elements of water is an effect of light, there are other cases in which, either by ferment action, or other powers residing in the plant, the same result can be attained.

810. Boussingault states that grass leaves dried in air at 212° Fah., and burned with oxide of copper, yield 1·3 per cent. of their dry weight of nitrogen, which nitrogen is, of course, in combination. I found, however, that there is, besides this, included in the tissue of the leaf a certain quantity of gas, which can be removed by the air-pump. I presume this air is naturally enclosed in the spiral vessels. When leaves are placed in an inverted jar with boiled water *in vacuo*, this gas is liberated; at first, most copiously from the fractured extremity; but as the process of exhaustion goes on, it exudes from both faces of the leaf, perhaps by rending open the frail tissue in which it is imprisoned. In leaves that have stomata on one side only, it does not pour forth from those organs in preference to other parts, and from this it may be inferred that it does not normally exist in the intercellular spaces. In a given weight of leaves its amount is very variable, ranging, in my experiments, from ·01 to ·02 cubic inch for ten grains of grass leaves. Its constitution, as determined by analysis, is also variable, but very remarkable; it contains from 88 to 94 per cent. of nitrogen.

811. It being, therefore, understood that in the tissue of the leaf a certain quantity of gas is mechanically included, which gas differs from atmospheric air in the circumstance that it contains a larger volume of nitrogen, which may be removed by the air-pump, we are in a condition to understand whether it is this nitrogen which furnishes the supply found in the gas exhaled by leaves. The following experiment proves that it is not:

812. I removed by continued boiling and exhaustion all the air dissolved in a solution of bicarbonate of soda. I also removed all the nitrogen from some grass leaves, by placing them *in vacuo* immersed in water that had been boiled and subsequently cooled. Then placing these leaves in the solution of the bicarbonate and in the vessels in which the experiment was finally to be conducted, I kept them *in vacuo* for an hour. This was done to get rid of that film of atmospheric air which always adheres to the surface of glass vessels, and which might have disturbed the result by furnishing nitrogen. The leaves were now exposed in the saline solution to the beams of the sun, and presently the evolution of gas commenced. When a sufficient quantity was collected, it was found to consist of 88 per cent. oxygen and 12 nitrogen.

813. Repetitions of this experiment prove that, although the nitrogen mechanically enclosed in the leaf to a certain extent mingles with the oxygen evolved, and, indeed, it could not be otherwise on account of the diffusion of gases into one another, yet the

true source is to be sought in some nitrogenized compound present in the leaf, which is undergoing decomposition in a regulated way.

814. Keeping this fact clearly before us, that the source of the nitrogen found thus in company with the oxygen, given off under the influence of light, is some nitrogenized body existing in the leaf, the following experiments will show the simple and beautiful law under which this phenomenon is conducted.

815. Saussure has already determined that when plants are forced to grow in an atmosphere of known volume, containing carbonic acid gas, after the decomposition of the gas is completed, the total volume remains unchanged. As my experiments were made with leaves immersed in water, I was desirous of proving whether, under these forced circumstances, the same result would still hold good.

816. To a certain quantity of water, from which all air had been expelled, confined in a jar over mercury, I passed 20 measures of carbonic acid gas; by a little agitation the water took up 15·50 measures of the acid. I now introduced into the jar some leaves, taking the greatest care that no bubbles of air should pass along with them. The jar was then placed in the sunshine, and the decomposition completed. Corrected for variation of temperature and pressure, the resulting volume of the gas in two experiments was 20, or precisely the same as that of the carbonic acid.

817. We may therefore infer that the volume of mixed gases evolved is precisely equal to the volume of carbonic acid that disappears. This leads us to some very remarkable conclusions.

818. When the leaves of plants, under the influence of light, decompose carbonic acid, they assimilate all the carbon, and a certain proportion of oxygen disappears, at the same time they emit a volume of nitrogen *equal to that of the oxygen consumed.*

819. This disappearance of oxygen and appearance of nitrogen are thus connected with each other: they are equivalent phenomena.

820. The emission of nitrogen is thus shown not to be a mere accidental result, but to be profoundly connected with the whole physiological action.

821. I arrive also at this conclusion from experiments of another kind. If the nitrogen that appears in company with oxygen were obtained by diffusion from gas mechanically shut up in the parenchyma of the leaf, it is plain, in the mode of operation which I have followed, in which leaves are immersed under water, and no opportunity given them of restoring their mechanically included air, if it were by any means withdrawn, that the first portions of mixed gas evolved should be richest in nitrogen, and that the per-centage amount should gradually become less and less, as it was removed from the structure of the leaf: this follows from the laws of the diffusion of gases. But this is far from being the case. It very commonly happens that more nitrogen is evolved *at the close of the process* than at its beginning. Thus, in one of the experiments I made, in which it was found that there was 22·2 per cent. of nitrogen in the total resulting volume, the quantities that had been evolved in three successive periods of examination, from the beginning to the termination of the experiment, were,

1st period,		21·8	per cent.	of nitrogen.
2d	"	18·8	"	"
3d	"	26·0	"	"

During the progress of this decomposition, therefore, more nitrogen, relatively, was evolved towards the close of the experiment than at its beginning.

822. From this result, therefore, I again infer that the nitrogen emitted by leaves is derived from the decomposition of some azotized body, and not from air mechanically included in their pores.

823. The following are the experimental results which I have obtained:

1st. That the nitrogen comes from the tissue of the leaf itself; because more than three times as much is evolved from bicarbonate of soda as is imprisoned in the structure of the leaf, removable by the air-pump.

2d. In twelve hours, from bicarbonate of soda, leaves will evolve more than five times their own volume of gaseous matter.

3d. The quantity of nitrogen in the composition of leaves is sufficient for furnishing all the nitrogen obtained in the gas evolved. From Boussingault's analyses it appears that they contain nearly ten times the required amount.

4th. The decomposition of some nitrogenized constituent of the leaf is essential to the appearance of the nitrogen: there is no other available source.

824. At this stage of the inquiry a remarkable analogy appears between the function of digestion in animals and the same function in plants. Liebig has shown how, from the transformation of the stomach itself, food becomes acted upon and is turned into chyme: an obscure species of fermentation, brought about by the action of nitrogenized bodies. So, in like manner, in plants, the decay of a nitrogenized body is intimately connected with the assimilation of carbon; for, as I have stated, the process here under discussion is a true digestion and not a respiratory process. And as there are facts which seem to show that the primary action of the light is not upon the carbonic acid, but upon the nitrogenized ferment, the decomposition of the gas ensuing as a secondary result, *is it not probable that* CHLOROPHYL *is the body which in vegetables answers to the* CHYLE *of animals?* The oxygen, which disappears during the decomposition of carbonic acid, disappears to bring about the eremacausis of the nitrogenized body. And have not the gum, the starch, the lignin, and other carbonaceous constituents of plants, all originally existed in and passed through the green stage? It is the quality of radiant matter to determine the position of atoms and the grouping of molecules; and for this the sun, the great organizer, the great life-giver, from age to age furnishes his unfading beams. That analogies like this between the organic functions of plants and animals in reality exist, we might reasonably suppose; they are agreeable to the general plan of nature.

Note on the Tithonotype.

825. In chapter xiv. I described a process for obtaining tithonotypes, or copies of the surface of Daguerreotypes, by means of gelatine.

826. A very important improvement on that process—an improvement which, indeed, has brought it almost at once to perfection—has been effected; this is, *To copy the sur-*

face in copper by the electrotype after it has been previously fixed by the agency of a film of gold.

827. Those who are conversant with these matters will see at once that this is a very different thing from the abortive attempts which were made early in the history of the Daguerreotype. Many artists endeavoured to transfer its surface by precipitating copper upon it; among others, I made trials of the kind. The results of those abortive attempts were mere shadowy representations, which could be seen in certain lights, and which were very unsatisfactory in their effect.*

828. The beautiful tithonotypes that are now so common in this city are made in the following way: The Daguerreotype is carefully gilt by M. Fizeau's process, taking care that the film of gold is neither too thick nor too thin. The proper thickness is readily attained after a little practice. The plate is then kept a day or two, so that it may become enfilmed with air. The back and edges being varnished, copper is to be deposited upon it in the usual way, the process occupying from twelve to twenty hours. If the plate has been properly gilt, and the process conducted successfully, the tithonotype readily splits off from the Daguerreotype.

829. The reader will understand that, when the process succeeds, the Daguerreotype will be uninjured, and the tithonotype a perfect copy of it. If any portions are blue, or white, or flesh-coloured, they will be seen *in the same colours* in the tithonotype; the intensity of light and shadow is also given with accuracy, and, indeed, the copy is *a perfect copy*, in all respects, of the original. A great advantage is also obtained in the reversal that takes place. The right side of the tithonotype corresponds to the right side of the original object, and the left to the left. In the Daguerreotype it is not so.

830. Copper tithonotypes were first made in this city by Mr. Endicott, a lithographic artist of distinction.

831. There is no great difficulty in obtaining from these tithonotypes duplicate copies. An expert artist can multiply them from one another.

832. The problem of multiplying the beautiful productions of M. Daguerre is therefore solved.

833. I will take this opportunity of making a remark which I intended to have inserted in my paper "On the rapid Detithonizing power of certain Gases and Vapours," inserted in the March number of the *Philosophical Magazine* (S. 3, vol. xxii.). Amateurs in the Daguerreotype process are often annoyed by the want of success which frequently attends them. They ascribe to the atmosphere, or to the light, or to other causes, their inability to obtain impressions. Most of these mischances are due to the accidental presence of the vapour of iodine, or other electro-negative bodies, in the chamber or about the apparatus. It is incredible what a brief exposure to these vapours will entirely destroy a picture before it is mercurialized. If the iodine box or the bromine bottle is kept in the same room with the mercury apparatus, that circumstance in itself is often sufficient to ensure a uniform want of success. If the little frame which fits into the back of the camera, and which holds the silver plate, be used in the iodi-

* Professor Grove's voltaic process for etching Daguerreotypes has, however, produced better results than those here alluded to by Dr. Draper. See *Phil. Mag.*, S. 3, vol. xx, p. 18.—Edit. Phil. Mag.

zing process, as is often the case, the small quantity of vapour it absorbs will destroy every picture, or, at all events, increase the time required in the camera enormously. The reason of this is easily understood. Suppose a plate in such a frame be placed in the camera, or, what comes to the same thing, suppose a particle of iodine has fallen into the camera, or that the wood has in any way absorbed an electro-negative vapour; as fast as the light makes its impression on the sensitive surface the vapour detithonizes it, and unless the light is quite intense or the exposure much prolonged, a very feeble proof, or no proof at all, will be obtained. In the same way, the difficulties are greatly increased in the process of mercurialization; for the temperature resorted to being high, if there is the least particle of iodine about the box, the picture will be inevitably and instantly detithonized and ruined.

834. We ought, therefore, never to allow iodine, or bromine, or chlorine, to have access to the apartment or the apparatus in which Daguerreotype operations are being conducted.

CHAPTER XVI.

DESCRIPTION OF THE TITHONOMETER, AN INSTRUMENT FOR MEASURING THE CHEMICAL FORCE OF THE INDIGO-TITHONIC RAYS.

(*From the London, Edinburgh, and Dublin Philosophical Magazine for December*, 1843.)

Contents: *The Instrument consists of a Mixture of Chlorine and Hydrogen.—It is acted upon by Lamp Light, an Electric Spark at a Distance, &c.—Chlorine and Hydrogen unite in Proportion to the Amount of Light.—Mode of measuring out known Quantities of Rays.—The Maximum of Action is in the Indigo Space.—Construction of the Instrument.—Theoretical Conditions of Equilibrium.—Preliminary Adjustment.—Method of continuous Observation.—Method of interrupted Observation.—Remarkable Contraction and Expansion.*

835. I have invented an instrument for measuring the chemical force of the tithonic rays which are found at a maximum in the indigo space, and which from that point gradually fade away to each end of the spectrum. The sensitiveness, speed of action, and exactitude of this instrument, will bring it to rank, as a means of physical research, with the thermo-multiplier of M. Melloni.

836. The means which have hitherto been found available in optics for measuring intensities of light, by a relative illumination of spaces or contrast of shadows, are admitted to be inexact. The great desideratum in that science is a photometer which can mark down effects by movements over a graduated scale. With those optical contrivances may be classed the methods hitherto adopted for determining the force of the tithonic rays by stains on Daguerreotype plates, or the darkening of sensitive papers. As deductions drawn in this way depend on the *opinion* of the observer, they can never be perfectly satisfactory, nor bear any comparison with thermometric results.

837. Impressed with the importance of possessing, for the study of the properties of the tithonic rays, some means of accurate measurement, I have resorted in vain to many contrivances; and, after much labour, have obtained at last the instrument which it is the object of this paper to describe.

838. The tithonometer consists, essentially, of a mixture of equal measures of chlorine and hydrogen gases, evolved from and confined by a fluid which absorbs neither. This mixture is kept in a graduated tube, so arranged that the gaseous surface exposed to the rays never varies in extent, notwithstanding the contraction which may be going on in its volume, and the muriatic acid resulting from its union is removed by rapid absorption.

839. The theoretical conditions of the instrument are, therefore, sufficiently simple; but, when we come to put them into practice, obstacles which appear at first sight insurmountable are met with. The means of obtaining chlorine are all troublesome; no liquid is known which will perfectly confine it; it is a matter of great difficulty to mix it in the true proportion with hydrogen, and have no excess of either. Nor is it at all an easy affair to obtain pure hydrogen speedily, and both these gases diffuse with rapidity through water into air.

840. Without dwelling farther on the long catalogue of difficulties which is thus to be encountered, I shall first give an account of the capabilities of the instrument in the form now described, which will show to what an extent all those difficulties are already overcome. In a course of experiments on the union of chlorine and hydrogen, some of which were read at the last meeting of the British Association, I found that the sensitiveness of that mixture had been greatly underrated. The statement made in the books of chemistry, that artificial light will not affect it, is wholly erroneous. The feeblest gleams of a taper produce a change. No farther proof of this is required than the tables given in this chapter, in which the radiant source was an oil-lamp. For speed of action, no tithonographic compound can approach it; a light which perhaps does not endure the millionth part of a second, affects it energetically, as will be hereafter shown.

841. *Proofs of the Sensitiveness of the Tithonometer.*—The following illustrations will show that the tithonometer is promptly affected by rays of the feeblest intensity, and of the briefest duration.

842. When, on the sentient tube of the tithonometer, the image of a lamp flame formed by a convex lens is caused to fall, the liquid instantly begins to move over the scale, and continues its motion as long as the exposure is continued. It does not answer to expose the tube to the direct emanations of the lamp without first absorbing the radiant heat, or the calorific effect will mask the true result. By the interposition of a lens this heat is absorbed, and the tithonic rays alone act.

843. If a tithonometer is exposed to daylight coming through a window, and the hand, or a shade of any kind, is passed in front of it, its movement is *in an instant* arrested; nor can the shade be passed so rapidly that the instrument will fail to give the proper indication.

844. The experimenter may farther assure himself of the extreme sensitiveness of

this mixture by placing the instrument before a window, and endeavouring to remove and replace its screen so quickly that it shall fail to give any indication; he will find that it cannot be done.

845. Charge a Leyden vial. and place the tithonometer at a little distance from it, keeping the eye steadily fixed on the scale; discharge the jar, and the rays from the spark will be seen to exert a very powerful effect, the movement taking place and ceasing in an instant.

846. This remarkable experiment not only serves to prove the sensitiveness of the tithonometer. but also brings before us new views of the powers of that extraordinary agent, electricity. That energetic chemical effects can thus be produced at a distance by an electric spark in its momentary passage, effects which are of a totally different kind from the common manifestations of electricity, is thus proved; these phenomena being distinct from those of induction or molecular movements taking place in the line of discharge, they are of a radiant character, and due to the emission of tithonicity; and we are led at once to infer that the well-known changes brought about by passing an electric spark through gaseous mixtures. as when oxygen and hydrogen are combined into water, or chlorine and hydrogen into muriatic acid, arise from a very different cause than those condensations and percussions by which they are often explained, a cause far more purely chemical in its kind. If chlorine and hydrogen can be made to unite silently by an electric spark passing outside the vessel which contains them, at a distance of several inches, there is no difficulty in understanding why a similar effect should take place with a violent explosion when the discharge is made through their midst, nor how a great many mixtures may be made to unite under the same treatment. A flash of lightning cannot take place, nor an electric spark be discharged, without chemical changes being brought about by the radiant matter emitted.

847. *Proofs of the Exactness of the Indications of the Tithonometer.*—The foregoing examples may serve to illustrate the extreme sensitiveness of the tithonometer; I shall next furnish proofs that its indications are exactly proportional to the quantities of light incident on it.

848. As it is necessary, owing to the variable force of daylight, to resort to artificial means of illumination. it will be found advantageous to employ the following method of obtaining a flame of suitable intensity:

849. Let A B (*fig.* 104) be an Argand oil lamp, of which the wick is C. Over the wick, at a distance of half an inch. or thereabout, place a plate of thin sheet copper, three inches in diameter, perforated in its centre with a circular hole of the same diameter as the wick, and concentric therewith. This piece of copper is represented at *d d;* it should have some contrivance for raising or depressing it through a small space, the proper height being determined by trial. On this plate the glass cylinder, *e*, an inch and three quarters in diameter, and eight or ten inches long, rests.

850. When the lamp is lighted, provided the distance between the plate, *d d*, and the top of the wick is properly adjusted, on putting on the glass cylinder the flame instantly assumes an intense whiteness; by raising the wick it may be elongated to six inches or more, and becomes exceedingly brilliant. Lamps constructed on these principles may

be purchased in the shops. I have, however, contented myself with using a common Argand study-lamp, supporting the perforated plate, *d d*, at a proper altitude by a retort stand. It will be easily understood that the great increase of light arises from the circumstance that the flame is drawn violently through the aperture in the plate by the current established in the cylinder.

851. As much radiant heat is emitted by this flame, in order to diminish its action, and also to increase the tithonic effect, I adopt the following arrangement: Let A B (*fig.* 104) be the lamp: the rays emitted by it are received on a convex lens, D, four inches and three quarters in diameter, that which I use being the large lens of a lucernal microscope. This, placed at a distance of twenty-one inches from the lamp, gives an image of the flame at a distance of thirteen inches, which is received on the sentient tube of the tithonometer, F; between the tithonometer and the lens there is a screen, E.

852. Things being thus arranged, and the lamp lighted so as to give a flame about three inches and a half long, the experiments may be proceeded with. It is convenient always to work with the flame at a constant height, which may be determined by a mark on the glass cylinder. At a given instant, by a seconds watch, the screen E is removed, and immediately the tithonometer begins to descend. When the first minute is elapsed, the position on the scale is read off and registered: at the close of the second minute the same is done, and so on with the third, &c. And now, if those numbers be compared, casting aside the first, they will be found equal to one another, as the following table of experiments, made at different times and with different instruments, shows:

TABLE I.

Showing that, when the radiant source is constant, the amount of movement in the tithonometer is directly proportional to the times of exposure.

Time.	Experiments. 1.	2.	3.	4.	5.
30″	7·00	7·00	10·25	. . .	5·25
60	8 00	8·75	11·50	11.75	6·50
90	7·50	8·00	11·50	. . .	6·25
120	7.75	7·75	11·50	13·00	6·00
150	7·75	7·25	.	. . .	6·00
180	. . .	. . .	.	12·00	6·00
210	. . .	. . .	. . .	. . .	6·00
Mean	7 60	7·55	11·19	12 25	6 00

From this it will be perceived that, taking the first experiment as an example, if at the end of 30 seconds the tithonometer has moved 7·00, at the end of 60″ it has moved 8·00 more; at the end of 90″, 7·50 more; at the end of 120″, 7·75 more: the numbers set down in the vertical column representing the amount of motion for each thirty seconds. And when it is recollected that the readings are all made with the instrument in motion, the differences between the numbers do not greatly exceed the possible errors of observation. It may be remarked that the third and fourth experiments were made with a different lamp.

853. Though a certain amount of radiant heat from a source so highly incandescent as that here used will pass the lens, its effects can never be mistaken for those of the tithonic rays. This is easily understood when we remember that the effect of such

transmitted heat would be to expand the gaseous mixture, but the tithonic effect is to contract it.

854. Next I shall proceed to show that the indications of the tithonometer are strictly proportional to the quantity of rays that have impinged upon it; a double quantity producing a double effect, a triple quantity a threefold effect, &c.

855. A slight modification in the arrangement (*fig.* 104) enables us to prove this in a satisfactory way. The lens, D, being mounted in a square wooden frame, can easily be converted into an instrument for delivering at its focal point, where the sentient tube is placed, measured quantities of the tithonic rays, and thus becomes an invaluable auxiliary in those researches which require known and predetermined quantities of tithonicity to be measured out. The principle of the modification is easily apprehended. If half the surface of the lens be screened by an opaque body, as a piece of blackened cardboard, of course only half the quantity of rays will pass which would have passed had the screen not been interposed. If one fourth of the lens be left uncovered, only one fourth of the quantity will pass; but in all these instances the focal image remains the same as before. By adjusting, therefore, upon the wooden frame of the lens two screens, the edges of which pass through its centre, and are capable of rotation upon that centre, we shall cut off all light when the screens are applied edge to edge; we shall have 90° when they are rotated so as to be at right angles, and 180° when they are superposed with their edges parallel. Thus, by setting them in different angular positions, we can gain all quantities from 0° up to 180°, and by removing them entirely away, reach 360°.

856. It will be understood that the effect of the instrument is to give an image of a visible object, of which the intensity can be made to vary at pleasure in a known proportion.

857. In order, therefore, to prove that the indications of the tithonometer are proportional to the quantity of impinging rays, place this *measuring lens* in the position D, setting its screens at an angle of 90°. Remove the screen E, and determine the effect on the tithonometer for one minute. At the close of the minute, and without loss of time, turn one of the screens so as to give an angle of 180°, and now the effect will be found double what it was before, as in the following table:

TABLE II.

Showing that the Indications of the Tithonometer are proportional to the quantity of Incident Rays.

Quantities.	Experiment 1. Observed.	Experiment 1. Calculated.	Experiment 2. Observed.	Experiment 2. Calculated.
90°	2·18	2 22	2·69	2·75
180	4·27	4 45	5·75	5·50
270	6·70	6·67	8·25	8·25
360	8 90	8 90	11·00	11 00

858. I have stated in the commencement of this paper, that the action upon the tithonometer is limited to a ray which corresponds in refrangibility to the indigo, or, rather, that in the indigo space its maximum action is found. The following table serves at once to prove this fact, and also to illustrate the chemical force of the different regions of the spectrum:

TABLE III.

Showing that the Maximum for the Tithonometer is in the Indigo Space of the Spectrum.

Space.	Ray.	Force.	Space.	Ray.	Force.
0	Extreme red	·33	8	Blue-indigo . .	204·00
1	Red . . .	·50	9	Indigo . . .	240 00
2	Orange . .	·75	10	Violet .	121·00
3	Yellow . .	2·75	11	Violet	72 00
4	Green . .	10·00	12	Violet	48 00
5	Green-blue	54·00	13	Violet	24·00
6	Blue . . .	108·00	14	Extra-spectral .	12·00
7	Blue . . .	144 00			

In this table the spaces are equal; the centre of the red, as insulated by cobalt blue glass, is marked as unity; the centre of the yellow, insulated by the same, being marked 3; the intervening region being divided into two equal spaces, and divisions of the same value carried on to each end of the spectrum.

859. As instruments will no doubt be hereafter invented for measuring the phenomena of different classes of rays, it may prove convenient to designate the precise ray to which they apply. Perhaps the most simple mode is to affix the name of the ray itself. Under that nomenclature, the instrument described in this paper would take the name of indigo-tithonometer.

860. There is no difficulty in adapting this instrument to the determination of questions relating to absorption, reflexion, and transmission. Thus, I found that a piece of colourless French plate-glass transmitted 866 rays out of 1000.

861. *Description of the Instrument. First, of the Glass Part.*—The tithonometer consists of a glass tube bent into the form of a siphon. in which chlorine and hydrogen can be evolved from muriatic acid, containing chlorine in solution, by the agency of a voltaic current. It is represented by *fig.* 105. where *a b c* is a clear and thin tube, four tenths of an inch external diameter, closed at the end, *a*. At *d*, a circular piece of metal an inch in diameter, which may be called the stage, is fastened on the tube, the distance from *d* to *a* being 2·9 inches. At the point *x*, which is two inches and a quarter from *d*, two platina wires, *x* and *y*, are fused into the glass, and, entering into the interior of the tube, are destined to furnish the supply of chlorine and hydrogen; from the stage, *d*, to the point *b*, the inner bend of the tube is 2·6 inches, and from that point to the top of the siphon, *c*, the distance is three inches and a half. Through the glass at *z*, three quarters of an inch from *c*, a third platina wire is passed; this wire terminates in the little mercury cup, *r*, and *x* and *y* in the cups *p* and *q* respectively.

862. Things being thus arranged, the instrument is filled with its fluid, prepared as will presently be described; and as the legs, *a b*, *b c*, are not parallel to each other, but include an angle of a few degrees, in the same way that Ure's eudiometer is arranged, there is no difficulty in transferring the liquid to the sealed leg. Enough is admitted to fill the sealed leg and the open one partially, leaving an empty space to the top of the tube, at *c*, of two and three quarter inches.

863. A stout tube, six inches long and one tenth of an inch interior diameter, *e f*, is now fused on at *c*. Its lower end opens into the main siphon tube; its upper end is turned over at *f*, and is narrowed to a fine termination, so as barely to admit a pin, but is not closed. This serves to keep out dust, and in case of a little acid passing out, it does not flow over the scale and efface the divisions. At the back of this tube a

scale is placed, divided into tenths of an inch, being numbered from above downward. Fifty of these divisions are as many as will be required. *Fig.* 106 shows the termination of the narrow tube bent over the scale.

864. From a point one fourth of an inch above the stage, *d*, downward beyond the bend, and to within half an inch of the wire, *z*, the whole tube is carefully painted with India ink, so as to allow no light to pass; but all the space from a fourth of an inch above the stage, *d*, to the top of the tube, *a*, is kept as clear and transparent as possible. This portion constitutes the sentient part of the instrument. A light metallic or pasteboard cap, A D (*fig.* 107), closed at the top and open at the bottom, three inches long and six tenths of an inch in diameter, blackened on its interior, may be dropped over this sentient tube; it being the office of the stage, *d*, to receive the lower end of the cap when it is dropped on the tube so as to shut out the light.

865. The foot of the instrument, *k l*, is of brass; it screws into the hemispherical block, *m*, which may be made of hard wood or ivory; in this three holes, *p*, *q*, *r*, are made to serve as mercury cups; they should be deep and of small diameter, that the metal may not flow out when it is inclined for the purpose of transferring. A brass cylindrical cover, L M, L M, may be put over the whole; when it is desirable to preserve it in total darkness, it should be blackened within.

866. *Secondly, of the Fluid Part.*—The fluid from which the mixture of chlorine and hydrogen is evolved, and by which it is confined, is yellow commercial muriatic acid, holding such a quantity of chlorine in solution that it exerts no action on the mixed gases as they are produced. From the mode of its preparation, it always contains a certain quantity of chloride of platina, which gives it a deep golden colour, a condition of considerable incidental importance.

867. When muriatic acid is decomposed by voltaic electricity, its chlorine is not evolved, but is taken up in very large quantity and held in solution; perhaps a bichloride of hydrogen results. If through such a solution hydrogen gas is passed in minute bubbles, it removes with it a certain portion of the chlorine. From this, therefore, it is plain that muriatic acid thus decomposed will not yield equal measures of chlorine and hydrogen, unless it has been previously impregnated with a certain volume of the former gas. Nor is it possible to obtain that degree of saturation by voltaic action, no matter how long the electrolysis is continued, if the hydrogen is allowed to pass through the liquid.

868. Practically, therefore, to obtain the tithonometric liquid, we are obliged to decompose commercial muriatic acid in a glass vessel, the positive electrode being at the bottom of the vessel and the negative at the surface of the liquid. Under these circumstances, the chlorine, as it is disengaged, is rapidly taken up, and the hydrogen being set free without its bubbles passing through the mass, the impregnation is carried to the point required.

869. Although this chlorinated muriatic acid cannot, of course, be kept in contact with the platina wires without acting on them, the action is much slower than might have been anticipated. I have examined the wires of tithonometers that have been in active use for four months and could not perceive the platina sensibly destroyed. It is

well, however, to put a piece of platina foil in the bottle in which the supply of chlorinated muriatic acid is kept: it communicates to it slowly the proper golden tint.

870. The liquid, being impregnated with chlorine in this manner until it exhales the odour of that gas, is to be transferred to the siphon, *a b c*, of the tithonometer, and its constitution finally adjusted as hereafter shown.

871. *Thirdly, of the Voltaic Battery.*—The battery which will be found most applicable for these purposes consists of two Grove's cells, the zinc surrounding the platina.

872. The following are the dimensions of the pairs which I use. The platina plate is half an inch wide and two inches long; it dips into a cylinder of porous biscuit-ware of the same dimensions, which contains nitric acid. Outside this porous vessel is the zinc, which is a cylinder one inch diameter, two inches long, and two tenths thick; it is amalgamated. The whole is contained in a cup two inches in diameter and two deep, which receives the dilute sulphuric acid.

873. The force of this battery is abundantly sufficient both for preparing the fluid originally and for carrying on the tithonometric operations; it can decompose muriatic acid with rapidity, and will last, with ordinary care, for a long time.

874. Before passing to the mode of using the tithonometer, it is absolutely necessary to understand certain theoretical conditions of its equilibrium; to these in the next place I shall revert.

875. *Theoretical Conditions of Equilibrium.*—The tithonometer depends for its sensitiveness on the exact proportion of the mixed gases. If either one or the other is in excess, a great diminution of delicacy is the result. The comparison of its indications at different times depends on the certainty of evolving the gases in exact, or, at all events, known proportions.

876. Whatever, therefore, affects the constitution of the sentient gases, alters, at the same time, their indications. Between those gases and the fluid which confines them certain relations subsist, the nature of which can be easily traced. Thus, if we had equal measures of chlorine and hydrogen, and the liquid not saturated with the former, it would be impossible to keep them without change, for, by degrees, a portion of chlorine would be dissolved, and an excess of hydrogen remain; or, if the liquid was overcharged with chlorine, an excess of that gas would accumulate in the sentient tube.

877. It is absolutely necessary, therefore, that there should be an equilibrium between the gaseous mixture and the confining fluid.

878. As has been said, when muriatic acid is decomposed by a voltaic current, all the chlorine is absorbed by the liquid, and accumulates therein; the hydrogen bubbles, however, as they rise, withdraw a certain proportion, and hence pure hydrogen passed up through the tithonometric fluid becomes exceedingly sensitive to the light.

879. There are certain circumstances connected with the constitution and use of the tithonometer which continually tend to change the nature of its liquid. The platina wires immersed in it, by slow degrees, give rise to a chloride of platina. It is true that this takes place very gradually, and by far the most formidable difficulty arises from a direct exhalation of chlorine from the narrow tube *e f*, for each time that the liquid descends, a volume of air is introduced, which receives a certain amount of chlorine,

which, with it, is expelled the next time the battery raises the column to zero; and this, going on time after time, finally impresses a marked change on the liquid. I have tried to correct this in various ways, as by terminating the end *f* with a bulb; but this entails great inconvenience, as may be discovered by any one who will reflect on its operation.

880. When by the battery we have raised the index to its zero point, if the gas and liquid are not in equilibrio, that zero is liable to a slight change. If there be hydrogen in excess, the zero will rise; if chlorine, the zero will fall.

881. In making what will be termed "interrupted experiments," we must not too hastily determine the position of the index on the scale at the end of a trial. It is to be remembered that the cause of movement over the scale arises from a condensation of muriatic acid, but that condensation, though very rapid, is not instantaneous. Where time is valuable, and the instrument in perfect equilibrium, this condensation may be instantaneously effected by simply inclining the instrument so that its liquid may pass down to the closed end *a*, but not so much as to allow gas to escape into the other leg; the inclination of the two legs to each other makes this a very easy manipulation, and the gas, thus brought into contact with an extensive liquid surface, yields up its muriatic acid in a moment.

882. *Directions for using the Tithonometer. Preliminary Adjustment.*—Having transferred the liquid to the sealed end of the siphon, and placed the cap on the sentient extremity, the voltaic battery being prepared, the operator dips its polar wires into the cups *p q*, which are in connexion with the wires *x y*. Decomposition immediately takes place, chlorine and hydrogen rising through the liquid, and gradually depressing it, while, of course, a corresponding elevation takes place in the other limb; this operation is continued until the liquid has risen to the zero. It takes but a few seconds for this to be accomplished.

883. The polar wires having been disengaged, the tithonometer is removed opposite a window, care being taken that the light is not too strong. The cap is now lifted off the sentient extremity, *a d*, and immediately the liquid descends. This exposure is allowed to continue, and the liquid suffered to rise as much as it will to the end *a*. And now, if the gases have been properly adjusted, an entire condensation will take place, the sentient tube *a d* filling completely. In practice, this precision is not, however, obtained; and if a bubble as large as a peppercorn be left, the operator will be abundantly satisfied with the sensitiveness of his instrument. Commonly, at first. a large residue of hydrogen gas, occupying, perhaps, an inch or more, will be left. It is to be understood that even this large surplus will disappear in a few hours by absorbing chlorine. But this is not to be waited for: as soon as no farther rise takes place in a minute or two, the siphon is to be inclined on one side, and the residue turned out into the open leg.

884. Now, recurring to what has been said on the equilibrium, it is plain that this excess of hydrogen arises from a want of chlorine in the tithonometric liquid. A proper quantity must, therefore, be furnished by proceeding as follows:

885. The sentient tube being filled with the liquid by inclination, connect the polar

wires with $p\ q$, as before. These may be called *generating wires.* Allow the liquid to rise in $b\ c$ until the third platina wire, z, which may be called the *adjusting wire*, is covered an eighth of an inch deep. Then remove the negative wire from the cup p into the cup r, and now the conditions for saturating the liquid are complete; hydrogen escaping away from the surface of the liquid at z, and chlorine continually accumulating and dissolving between x and d. This having been carried on for a short time, the gas in $a\ d$ is to be turned out by inclination, and the instrument recharged. That a proper quantity is evolved is easily ascertained by allowing total condensation to take place, and observing that only a small bubble is left at a.

886. It will occasionally happen in this preliminary adjustment, that an excess of chlorine may arise from continuing the process too long. This is easily discovered by its greenish-yellow tint, and is to be removed by inclining the instrument and turning it out.

887. Thus adjusted, everything is ready to obtain measures of any effect, there being two different methods by which this can be done: 1st, by continuous observation; 2d, by interrupted observation.

888. *Of the Method of Continuous Observation.*—This is best described by resorting to an example. Suppose, therefore, it is required to verify Table I., or, in other words, to prove that the effect on the tithonometer is proportioned to its time of exposure.

Put on the cap of the sentient tube $a\ d$, connect the polar wires with $p\ q$, and raise the liquid to zero.

Place the tithonometer so that its sentient tube will receive the rays properly.

At a given instant, marked by a seconds watch, remove the cap A D, and the liquid at once begins to descend. At the end of the first minute read off the division over which it is passing. Suppose it is 7. At the end of the second do the same, it should be 14; at the end of the third, 21, &c., &c. This may be done until the fiftieth division is reached, which is the terminus of the scale.

Recharge the tube by a momentary application of the polar wires: but it is convenient first to remove any excess of muriatic acid gas in the sentient tube, by allowing it time for condensation; or, if that be inadmissible, by inclining a little on one side, so as to give an extensive liquid contact.

889. *Of the Method of Interrupted Observation.*—It frequently happens that observations cannot be had during a continuous descent, as when changes have to be made in parts of apparatus or arrangements. We have, then, to resort to interrupted observations.

This method requires that the gas and liquid should be well adjusted, so that no change can arise in volume when extensive contact is made by inclination.

The tithonometer being charged, place it in a proper position. At a given instant remove its cap, and the liquid descends. When the time marked by a seconds watch has elapsed, drop the cap on the sentient tube. The liquid simultaneously pauses in its descent, but does not entirely stop, for a little uncondensed muriatic acid still exists, which is slowly disappearing in the sentient tube. Now incline the instrument for a moment on one side, so that the liquid may run up to the end a, but not so much as to

let any gas escape. Restore it to its position, and read off on the scale. It is then ready for a second trial.

890. The difference between continuous and interrupted observation is this, that in the latter we pause to wash out the muriatic acid; and though this is effected by the simplest of all possible methods, continuous observations are always to be preferred when they can be obtained.

891. I have extended this paper to so great a length that many points on which remarks might have been made must be passed over. It is scarcely necessary to say that the sentient tube must be *uniformly* and perfectly clean. As a general rule, also, the first observation may be cast aside, for reasons which I will give hereafter. Farther, it is to be remarked, as it is an essential principle that during different changes of volume of the gas its exposed surface must never vary in extent, the liquid is not to be suffered to rise above the blackened portion at *d*. If the measures of the different parts be such as have been here given, this cannot take place, for the liquid will fall below the fiftieth division before its other extremity rises above *d*.

892. The same original volume of gas in *a d* will last for a long time, as we keep replenishing it as often as the fiftieth division is reached.

893. The experimenter cannot help remarking, that on suddenly exposing the sentient tube to a bright light, *the liquid for an instant rises* on a scale, and on dropping the cap, *in an instant falls.* This important phenomenon, which is strikingly seen under the action of an electric spark, I shall consider hereafter.

894. In conclusion, as to comparing the tithonometric indications at different times, if the gases have the same constitution, the observations will compare; and if they have not, the value can from time to time be ascertained by exposure to a lamp of constant intensity. To this method I commonly resort.

895. From the space occupied in this description the reader might be disposed to infer that the tithonometer is a very complicated instrument, and difficult to use. He would form, however, an erroneous opinion. The preliminary adjustment can be made in five minutes, and with it an extensive series of measures obtained. These long details have been entered into that the theory of the instrument may be known, and optical artists construct it without difficulty. Though surprisingly sensitive to the action of the indigo ray, it is as manageable by a careful experimenter as a common differential thermometer.

CHAPTER XVII.

ON TITHONIZED CHLORINE.

CONTENTS: *Description of the Experiment.—The Change in the Chlorine is not Transient.—There are two Stages in the Phenomenon.—Rays are absorbed in producing this Change.—It is the Indigo Ray which is absorbed.—The Action is positive from End to End of the Spectrum.—The Indigo Ray forms Muriatic Acid, as well as produces the Preliminary Tithonization.—Change in other Elementary Bodies.—Verification of the preceding Results with the Tithonometer.*

(*From the London, Edinburgh, and Dublin Philosophical Magazine for July,* 1844.)

896. CHLORINE gas, which has been exposed to the daylight or to sunshine, possesses qualities which are not possessed by chlorine which has been made in the dark.

897. This is shown by the circumstance that chlorine which has been exposed to the sunshine has obtained from that exposure the property of speedily uniting with hydrogen gas; a property not possessed by chlorine which has been made and kept in the dark.

898. This quality gained by the chlorine arises from its having absorbed tithonic rays corresponding in refrangibility to the indigo. It is not a transient, but apparently a permanent property, the rays so absorbed becoming latent, and the effect lasting for an unknown period of time. The facts which I shall proceed to describe will be interesting to chemists, because they plainly lead us to suspect that the descriptions we have of the properties of all elementary and compound bodies are either inaccurate or confused. These properties are such as bodies exhibit after they have been exposed to the light; we still require to know what are the properties they possess before exposure to such influences.

899. Natural philosophers will also find an interest in these phenomena, for they finally establish for the tithonic rays two important facts: 1st, that those rays are absorbed by ponderable bodies; and, 2d, that they become latent after the manner of heat. Some years ago I endeavoured to prove that these things held for a compound substance the iodide of silver (*Phil. Mag.*, Sept., 1841).

900. For reasons which will be obvious as the description proceeds, I shall speak of chlorine which has been exposed to the beams of the sun as *tithonized chlorine.*

I. *Description of the Experiment.*

901. In two similar white glass tubes place equal volumes of chlorine, which has been made from peroxide of manganese and muriatic acid by lamplight, and carefully screened from access of daylight. Expose one of the tubes to the full sunbeams for some minutes, or, if the light be feeble, for a quarter of an hour: the chlorine which is in it becomes tithonized. Keep the other tube during this time carefully in a dark place; and now, by lamplight, add to both equal volumes of hydrogen gas. These

processes are best carried on in a small porcelain or earthenware trough, filled with a saturated solution of common salt, which dissolves chlorine slowly; and to avoid explosions, operate on limited quantities of the gases. Tubes that are eight inches long and half an inch in diameter will answer very well. The two tubes now contain the same gaseous mixture, and only differ in the circumstance that one is tithonized and the other not. Place them, therefore, side by side before a window, through which the entrance of daylight can be regulated by opening the shutter; and now, if this part of the process is conducted properly, it will be seen that the tithonized chlorine commences to unite with the hydrogen, and the salt water rises in that tube. But the untithonized chlorine shows no disposition to unite with its hydrogen, and the liquid in its tube remains motionless for a long time. Finally, as it becomes slowly tithonized by the action of the daylight impinging on it, union at last takes place. From this, therefore, we perceive that chlorine which has been exposed to the sun will unite promptly and energetically with hydrogen; but chlorine that has been made and kept in the dark shows no such property.

902. As I doubt not this remarkable experiment will be repeated by chemists, I will add that the only point to which attention in particular is to be given, is in the final exposure to the light. This must not be too feeble, or the action will be tedious; but the direct sunbeam must be sedulously excluded, or an explosion will result. A room illuminated by one small window, looking to the north, answers very well. It need scarcely be added, that care must be taken that both tubes are illuminated alike.

II. *The Change in the Chlorine is not Transient.*

903. Now it might be supposed that this apparent exaltation of the electro-negative properties of the chlorine is only a transient thing which would speedily pass away, the gas reverting to its original untithonized condition.

904. To show that this is not so, tithonize some chlorine in a tube as before. Place it for an hour or two in the dark along with the tube of untithonized chlorine, with which it is to be compared; then to both add hydrogen. Expose them, as in the former experiment, to the daylight, and the result will turn out as before, the tithonized chlorine forming muriatic acid at once, and the untithonized refusing to do so.

905. This, therefore, shows that the change which the sunbeams impress upon chlorine is to a certain extent a permanent change, and, unlike a calorific effect, it does not spontaneously and rapidly pass away.

III. *There are two Stages in the Phenomenon.*

906. Let us now proceed to make an inquiry into the nature of the change thus impressed on the chlorine. This, I shall show, rests in the circumstance of the absorption of rays which correspond in refrangibility to the indigo, and which appear to become latent.

907. In a tube, over salt water, mix together equal volumes of untithonized chlorine and hydrogen gas. Expose it to the daylight, marking the time at which the exposure commences. Watch the level of the liquid in the tube narrowly, and, though station-

ary for a considerable time, after a certain period has elapsed it will be seen on a sudden to start and commence rising. Observe now how far it will rise during a period which is equal to the time that elapsed between the first exposure and the beginning of the rise, and it will be seen that one fourth or half of the gases will disappear.

908. It is obvious that from the first moment of exposure the rays must have been exerting their influences on the mixture. As will presently be proved, absorption has been all along taking place. There are, therefore, two distinct phenomena exhibited by this experiment. There is a period during which, though large quantities of the dark rays are disappearing, no visible change is produced; there is a second period, during which absorption is accompanied by a remarkable chemical effect, the production of muriatic acid. From these things we gather that a definite amount of the tithonic rays must disappear and become latent before muriatic acid can form. The phenomenon is not unlike that of the disappearance of a definite quantity of heat in the passage of ice into the condition of water.

909. A mixture of chlorine and hydrogen does not, therefore, instantly give rise to the production of muriatic acid on exposure to the light, but, as a preliminary condition, a certain definite amount of absorption must take place.

910. Now if this were a mere molecular disturbance, such as might be brought about by the action of heat, we should expect to find it transient and speedily passing away. Such, however, is far from being the case. As with simple chlorine, so with this mixture, after it has been tithonized it loses its quality very slowly. I have observed that after a week or more has elapsed since it was first exposed to the light, it commences to contract when placed in a feeble gleam.

IV. *Rays are absorbed in producing this Change.*

911. I have thus far assumed that the rays which bring about these are absorbed; the following is the proof which I have to offer:

912. Over a tube half an inch in diameter and six inches long, closed at its upper extremity and open at its lower, invert a jar of the same length and one inch and a half in diameter. Fill the tube and the jar at the salt water trough, about two thirds full, with the same mixture of chlorine and hydrogen. Expose them to diffuse daylight. Now it is clear that no rays can gain access to the tube, except after having passed through the gaseous mixture in the jar. After a certain space of time the level of the liquid in the jar commences to rise, but that in the tube will remain much longer wholly stationary.

913. It therefore appears that a beam which has passed through a mixture of chlorine and hydrogen has lost, to a great extent, the quality of bringing about the union of a second portion of the mixed gases through which it may be caused to traverse. The active rays have been absorbed; they disappear from the beam, and are lost in producing their first effect.

914. A beam of light becomes detithonized in producing a chemical effect; the beam, as well as the medium on which it acts, becomes changed. I have a series of results which proves that this takes place for a great variety of compound bodies.

V. *It is the Indigo Ray which is absorbed.*

915. As has been said, it is a ray which corresponds in refrangibility to the indigo which produces these results.

916. In a small porcelain trough I inverted, side by side, ten tubes, each of which was three inches long and one third of an inch in diameter, the trough being filled with salt water. I passed into each tube a certain quantity of untithonized chlorine and hydrogen. A beam of the sun, being directed by a heliostat into a dark room, was dispersed horizontally by a flint glass prism, and the trough with its tubes so placed as to offer an exposure to the different coloured rays. The aperture which admitted the beam was about half an inch in diameter. For a while no movement was observed in any of the tubes; but as soon as the preliminary absorption, previously described, was over, and the tithonization completed, the level of the liquid began to rise. In the red and in the orange no movement could be perceived, in the violet only after a time; but first of all the tube that was immersed in the indigo light was in action, and exhibited finally a very rapid rise; this was soon followed by the tube that was in the space where the indigo and violet joined, then by that in the violet, and that in the blue; the tube in the green was next in order. The following table gives the numerical results obtained by observing the time which elapsed before movement took place in each tube:

TABLE I.

Name of Ray.	Time.	Name of Ray.	Time.
Extreme red	*	Indigo	1 50
Red and orange . . .	+100·00	Indigo and violet . . .	2 00
Yellow and green . . .	52·00	Violet	2 25
Green and blue . . .	4·00	Violet	5·00
Blue	2·33	Extreme violet . . .	5 50

917. Many years ago, M. Berard made experiments on the explosion of chlorine and hydrogen, and concluded, from his results, that it was brought about by the violet ray. This was at a time when the methods of making these experiments were less exactly known. It is a very easy matter to prove that, in reality, the indigo is the active ray, and that, from a maximum point which is in the indigo, but towards the blue, the effect gradually diminishes to each end of the spectrum.

918. The following table gives the calculated approximate intensity of the chemical force for each ray, deduced from the foregoing experiment:

TABLE II.

Name of Ray.	Force.	Name of Ray.	Force.
Extreme red		Indigo	66 60
Red and orange . . .	1·00	Indigo and violet . . .	50 00
Yellow and green . . .	1·90	Violet	44 40
Green and blue . . .	25·00	Violet	20·00
Blue	42·90	Extreme violet . . .	18 10

919. There is a great advantage which experiments conducted in this way possess over those which depend for their indication on the stains impressed on Daguerreotype plates or sensitive papers. In those cases we obtain merely a comparative contrast for different regions of the spectrum; in this we have absolute measures determined by a

* Even after the longest exposure I had the means of giving it, no movement took place in the tube which was in the extreme red, and I am doubtful about that in the red and orange.

definite chemical effect, and the rise of a liquid in a graduated tube; and from this we gain juster views of the true constitution of the spectrum. On studying the numbers in the foregoing table, or better still, if we project them, it will appear what an enormous difference there is in the chemical force of the different rays. In the experiment from which I have deduced this table, it appears that the force of the indigo ray exceeds that of the orange in a greater ratio than 66 to 1; and from the circumstances under which the experiment is made, this difference must be greatly underrated. There is always diffused light in the room coming from the intromitted beam, and this accelerates the rise in the less refrangible tubes; then, again, it is impossible that the tube which gives the greatest elevation shall coincide mathematically with the maximum point and express the maximum effect.

920. From some estimates I have made, I am led to believe that, in point of chemical force, for this mixture of chlorine and hydrogen, the indigo ray exceeds the red in a higer ratio than 500 to 1.

VI. *The Action is positive from End to End of the Spectrum.*

921. M. Becquerel found that for a Daguerreotype plate, the red, the orange, and the yellow rays possess the quality of continuing the action begun by the more refrangible colours; he therefore names these "*rayons continuateurs.*" For the same compound I found that those rays, acting conjointly with the diffused daylight, exerted a negative agency. It is therefore desirable to understand whether, with respect to the gases now under consideration, the lesser refrangible rays exert anything in the way of an action of depression or hinderance to union. By direct experiment, I found that this was not the case, the action being positive from end to end of the spectrum. This can be shown by removing the tubes, after they have been in the spectrum for an hour or two, into the gleams of daylight. One by one they exhibit after a time a rise, the order being the green first, then the yellow and the orange, and at last the red. And if, at the same time, a tube which has been kept in the dark be exposed along with them, they will all rise before it, showing that tithonization had set in and been going on in them all; that it had been more active in the green than in the yellow, in the yellow than in the orange, in the orange than in the red; and had the exposure to the spectrum been long enough, the liquid in every one of the tubes would have risen.

VII. *The Indigo Ray forms the Muriatic Acid, as well as produces the preliminary Tithonization.*

922. It only now remains to inquire, whether the rays which cause the production of the muriatic acid are those which effect the tithonization of the chlorine; in other words, whether the first stage of the process is brought about by the same agent which carries on the second. The experiment which I have just described shows that tithonization is most actively produced by the indigo ray, and it is easy to show that it is the same ray which carries on the second part of the process; for if, before placing the tubes in the prismatic spectrum, we tithonize them in the daylight, so that the liquid has just commenced to rise in each, and then expose them to the spectrum, it will be

found that the tube in the indigo rises most rapidly, and the others in the order stated before. Therefore we perceive that the same ray commences, carries on, and completes the process.

923. Few substances can exceed in sensitiveness to light a mixture of chlorine and hydrogen previously tithonized. Brought into the obscure daylight of a gloomy chamber, it is remarkable how promptly the level of the liquid in the tube rises; how, when the shutters are successively thrown open, the action becomes more and more energetic; and how, in an instant, it stops when the instrument is shaded by a screen.

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924. I have not recorded in this communication a multitude of experiments of detail, which go to support the conclusions here drawn, and which will be published at a proper time. It has been my object on this occasion to call attention to the fact, that chlorine, an elementary body, undergoes a change after exposure to the light; a change which appears to produce an exaltation of its electro-negative properties, as is shown by its power of uniting more energetically with hydrogen. This change must not be confounded with those transient elevations of activity due to increased temperature, inasmuch as this is more permanent in its character. It arises from the absorption of rays, which exist most abundantly in the indigo space of the spectrum. That the phenomenon is due to a true absorption, is fully shown in the circumstance, that a beam which has produced this effect has lost the quality of ever after producing a similar result. This is borne out by what we observe to take place when a feeble light falls on a mixture of chlorine and hydrogen which has been prepared in the dark. A certain space of time elapses before any formation of muriatic acid occurs, during which the absorption in question is going on; and when that is completed, and the mixture is tithonized, union of the gases begins, and muriatic acid forms. From end to end of the spectrum the action is positive, and differs only in intensity; but this difference in intensity opens before us new views of the constitution and character of the solar beam.

University of New-York, June 20, 1843.

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925. The foregoing paper was written almost a year ago, and since that time I have made several new observations corroborative of the results given.

926. Chlorine is not the only elementary substance in which the rays produce a change. In his chapter on phosphorus, Berzelius remarks, "Light produces in it (phosphorus) a peculiar change, of which the intimate nature is unknown; and which, so far as we can judge at present, does not alter its weight. It makes it take a red tint. This phenomenon occurs not only in a vacuum, even in that of a barometer, but also in nitrogen gas, in carburetted hydrogen, under water, alcohol, oil, and other liquids. When we expose to the sunlight phosphorus dissolved in ether, oil, or hydrogen gas, it instantly separates under the form of red phosphorus; it undergoes very rapidly this modification in *violet light*, or in glass vessels of a violet colour. The light of the sun makes it easily enter into fusion in nitrogen gas, but it do s not melt in hydrogen, and in the Torricellian vacuum it sublimes in the form of brilliant red scales."—(Berzelius, *Traité*, tom. i., p. 258.)

927. Again, when speaking of phosphuretted hydrogen, he says, "Exposed to the influences of the direct solar light this gas is decomposed, a part of the phosphorus separates under the form of red phosphorus, and is deposited on the interior surface of the glass. If we cover the vessel which contains the gas imperfectly, no phosphorus is deposited on the covered spaces."—(*Ib.*, tom. i., p. 265.)

928. As Berzelius does not give these experiments as his own, and I do not know to whom we are indebted for them, I repeated some of them. Among other corroborative results, it appeared that a piece of phosphorus of a pale or whitish colour, in a vessel filled with pure and dry carbonic acid gas, placed in the sunshine, exhibited the phenomenon in question. Eventually the phosphorus became of a deep blood-red colour, and on the sides of the glass towards the light feathery crystals formed, the tint of which bore a close resemblance to that of the red prussiate of potash.

929. Since the invention of the tithonometer, I have been able to observe more closely the habitudes of chlorine. In the description given of that instrument in Chapter XVI., it is recommended to cast aside the first observation, because it never gives an accurate estimate of the true effect. When a mixture of chlorine and hydrogen is exposed, muriatic acid does not immediately form; but a preliminary tithonization is necessary, and then, at the end of a certain period, contraction begins to take place.

930. A tithonometer exposed to the daylight is much too powerfully affected to allow of the successive stages of change to be distinctly made out; the preliminary tithonization is accomplished so rapidly, that the indications of it are merged and lost in the contraction which instantly follows. It is necessary, therefore, that we should operate with a *small* lamp-flame.

931. To such a flame I exposed a mixture of chlorine and hydrogen, and marked the number of seconds which elapsed before contraction, arising from the production of muriatic acid, took place. The first indications of movement occurred at the close of 600 seconds.

The index then	moved	through the	first	degree in	480	seconds.
"	"	"	second	"	165	"
"	"	"	third	"	130	"
"	"	"	fourth	"	95	"
"	"	"	fifth	"	93	"
"	"	"	sixth	"	93	"

and continued to move with regularity at the same rate.

932. These observations, therefore, prove that a very large amount of radiant matter is absorbed before chemical combination takes place; and that in the case of chlorine and hydrogen, the total action is divisible into two periods: the first, during which a simple absorption is taking place without a chemical effect; the second, during which absorption is attended with the production of muriatic acid.

933. The facts which I am endeavouring to set forth prominently in this communication are, 1st, the preliminary tithonization just discussed; and, 2d, the persistent character of the change impressed upon chlorine when it has been exposed to the sun, an effect wholly unlike a calorific effect. which would soon disappear.

934. By resorting to the tithonometer, we obtain information equally distinct upon the second point, that the preliminary tithonization is not a transient effect which at once passes away, but is, on the contrary, a persistent change.

935. I tithonized the chlorine and hydrogen contained in the instrument, and kept it in the dark for ten hours. On exposure to the lamp-rays it moved after a few seconds, showing, therefore, that the change which had been impressed on the chlorine was not lost. In the former case, 600 seconds had elapsed before any movement was visible.

936. When, however, we remember that the invisible images on Daguerreotype plates, and even photographic impressions on surfaces of resin, and probably all other similar changes, are slowly effaced, it would be premature to conclude that tithonized chlorine does not revert to its original condition. I have sometimes thought that there were in several of my experiments indications that this was taking place, but would not be understood to assert it positively. Whether it be so or not, one thing is certain, that the taking on of this condition and the loss of it is a very different affair from any transient exaltation of action due to a temporary elevation of temperature, or the contrary effect produced by cooling.

CHAPTER XVIII.

FARTHER CONSIDERATIONS ON THE EXISTENCE OF A FOURTH IMPONDERABLE.

CONTENTS: *Defects of former Evidence.—A new Photometer.—Measures of the Light transmitted by Coloured Solutions.—Explosion of Chlorine and Hydrogen by a distant Electric Spark.—Absorptive Action of Media.*

The Absorptive Action on Light and the Tithonic Rays follows different Laws.

Opacity of Glass for Phosphoric Rays.—Determination of the Refrangibility of the Phosphoric Rays of an Electric Spark.—Refrangibility of the same Rays in the Voltaic Arc of Flame.—Professor Henry's Experiments.

These Facts serve to prove that there are more than three Imponderables.

(*From the London, Edinburgh, and Dublin Philosophical Magazine for August*, 1844.)

937. In the Philosophical Magazine for December, 1842, I brought forward several facts which had caused me to form the opinion that the chemical rays of the older optical writers constitute, in reality, a new imponderable substance, which should be placed in the same rank with light, heat, and electricity. To the views then given, I propose, in this communication to return again, and furnish farther proof of their correctness. An extended examination. which has occupied me several years, has served to deepen my conviction of the truth of this doctrine.

938. Great changes in the fundamental theories of science ought not to be lightly

admitted. It was only after many years of discussion, and multitudes of experiments, that the doctrine of the unity of air was destroyed, and the theory of the intrinsic differences of gaseous bodies received. This was unquestionably the most important event that ever happened to chemistry.

939. The imponderable principles are the true LIVING FORCES of chemistry. The circumstance that they do not exhibit the property of weight is only an incidental affair, and ought never to have been regarded as their leading characteristic: they are the regulating forces by which ponderable matter is arranged and grouped. If, then, so great a change occurred in chemistry, on more exact views being obtained of its pneumatic department, what may not be expected from the discussions which are arising on the nature of its great controlling forces!

940. There is another point of view from which these investigations assume a deep interest. I have shown in Chapter XV. that, by resorting to prismatic analysis in physiological researches, very remarkable truths appear. The function of digestion, which is carried on during sunshine by the leaves of plants, is under the control of the yellow ray. It is this which causes the decomposition of carbonic acid, furnishes solid food, and gives the green colour. In animals similar results are produced by the agency of a nervous system; and not only so, but all the various operations connected with life are conducted in the same way. There is one class of nerves which gives action to the respiratory apparatus, and another which controls digestion. There is one class which presides over motion, another which is the recipient of sensation, a third which originates all the processes of thought and intellectuality. In the vegetable world the same idea is preserved, developed, perhaps, in a less elaborate way, but under the guidance of a principle equally ethereal and refined. *The beams of the sun are the true* NERVOUS PRINCIPLE *of plants.* To the yellow ray is assigned their nutritive processes, to the blue, their movements. We can, therefore, easily understand how it is that botanists, who have sought in the interior of plants for indications of a nervous agent, have never found them. That agent is external.

941. By the experiments that have been made for determining the nature of the chemical radiations, the question has been brought down to very narrow limits. There is no author who regards them as connected in any way with radiant heat, nor any, except the wildest speculator, who traces them to electricity. The difficulty is, to offer clear and undoubted proof that they are distinct from light. HERSCHEL has directly admitted this distinction, and brought forward several experiments (*Phil. Trans.*, 1840, p. 38, &c.) in support of this view. I have given some evidence of the kind, both recently and also several years ago (1837). All these experiments depend on a comparison of tithonographic stains produced by solar spectra that have undergone the action of absorptive media, and the effect of those spectra on the organ of vision; a comparison, in short, of different visible spectra and their tithonographic impressions.

942. From this comparison, we endeavour to prove that invisible rays may be isolated in any part of the spectrum, and, if invisible, we argue that they are not light.

943. It cannot be concealed, however, that there is a certain degree of imperfection in this species of evidence: an accurate conclusion as to the presence or absence, or

quantity of light, is by no means, under these circumstances, an easy affair. In these distorted spectra, as in the natural one, the terminations shade off gradually, and it is difficult to say where the light in reality ends. On these terminations also, where the light is so dilute and feeble, tithonographic action, although faint, may, by prolonged exposure, become not only perceptible, but even prominent. In tithonographic action, time enters as an element; in the act of vision it does not. A feeble gleam does not become more bright by constantly looking at it, but a sensitive surface exposed to such a gleam is more and more affected as the time is increased.

944. Considerations like these demonstrate the necessity of investigating the question in other ways, and more especially since M. Becquerel, one of the ablest writers on these matters, has undertaken to support a doctrine which denies the existence of the chemical rays, and imputes the whole action to light.—(*Taylor's Scientific Memoirs*, vol. iii., part 12.)

945. This doctrine, however, will not, I am persuaded, stand the test of criticism: there are facts, and these very imposing ones, which make it utterly untenable. It is my object, in this paper, to set forth that evidence, and offer farther and clearer proof of the physical independence of the tithonic rays and light, and indirectly establish the existence of a new imponderable.

946. The true issue of the question, as has been said, rests in proving a clear distinction between light and the tithonic rays; the other imponderables may be left out of the argument. The mechanical properties of the two agents are so closely alike—reflexion, refraction, polarization, interference, &c.—taking place under the same laws for both, that the discussion necessarily becomes one of quantity and measure. Will a given ray of light, disturbed by the action of absorptive media, change its luminous and chemical relations *pari passu?* or can we alter the one and leave the other untouched? Or, changing both by any process of treatment, do both change to the same extent?

947. The final decision of this question obviously rests in obtaining accurate measures for the rays of light and for the tithonic rays. It is the comparison of those measures which is to settle the point.

948. In Chapter XVI. I have described an instrument, under the name of the tithonometer, which gives indications by the production of muriatic acid from the union of chlorine and hydrogen. This instrument is affected chiefly by the indigo rays, or, more correctly speaking, by those rays which extend over the blue, indigo, and violet spaces of the spectrum, having their maximum in the indigo. It is important that the reader should keep this fact in mind.

949. Optical writers have been greatly embarrassed for want of a photometrical instrument which can measure the intensity of light; the chief difficulty in the way is the impossibility of contrasting together lights that differ in colour. By all, it is admitted that the eye is able to judge of the amount of illumination of white surfaces, or the depth of shadows within small limits of error, provided the rays compared are nearly of the same tint.

950. But in the discussion on which I am now entering, this very difficulty is in-

creased a hundred-fold. We are required to measure the intensity of light which has passed through all sorts of absorbent media, and, therefore, has become excessively coloured. How shall we compare together the rays which have gone through sulpho-cyanate of iron, and are of a deep blood-red, with those that have passed through sulphate of copper, and are of a bright blue?

951. Nevertheless, this problem is capable of a complete solution, and a photometer can be obtained which gives results comparable with those of the tithonometer: such an instrument I have constructed; it is exceedingly simple, as the following considerations prove.

952. We are to remember that the tithonometer gives indications which are expressive of the intensity of the blue rays generally; the blue tithonic rays are the rays which it measures. In using the term *blue*, it will be understood to comprehend the blue, indigo, and violet, or the more refrangible rays generally. It is obvious, therefore, that the photometer which is to be used with it must measure the same blue rays, or, in other words, the tithonometer and the photometer must be affected by rays comprehended between the same limits of refrangibility.

953. This can be effected by interposing in the photometer some absorbent medium, which will admit no rays to pass it except such as are in the limits of refrangibility with which the tithonometer is engaged. It is fortunate, as I have found, that such a medium occurs in a solution of sulphate of copper and ammonia.

954. Let a wooden box, *a b* (*fig.* 131), six inches long, two wide, and two deep, be provided; in the centre of its top an aperture three quarters of an inch in diameter is to be made; the box must be blackened interiorly, and a rectangular prism of wood, *c*, be placed in the box, with its right angle in such a position that its edge bisects as a diameter the circular aperture; over this wooden prism a piece of clean white paper should be pasted, care being taken that, where it bends over the right angle of the prism, it is folded sharp. So far the reader will recognise in this Ritchie's photometer, as described in the Annals of Philosophy. Upon the aperture in the top of the box a glass trough, *g h*, is placed; it is made by drilling a circular hole an inch in diameter in a piece of plate glass one third of an inch thick, and then placing on each side of it a thin piece of plate glass. This forms a circular trough, in which a strong solution of sulphate of copper and ammonia may be enclosed; over the trough a conical tube, *d*, six or eight inches long, is placed, so that the eye may see distinctly, through the aperture in the top of the box, the disk of paper, and more especially its dividing diameter.

955. Two small lamps, *e f*, are then prepared, of such dimensions that, when set opposite the open ends of the box, their rays may illuminate the paper; they are supposed to be adjusted so as to shine with equal intensity.

956. On looking through the tube a circle of blue light is seen, and, if the lamps are shining equally, its two halves are equally bright. At the commencement of every experiment this preliminary observation should be made, and, if necessary, the proper ad justments secured.

957. Suppose, now, it were required to know how much blue light is transmitted by

a given solution. A trough, *g*, is to be provided, which may be formed by drilling a hole two and a half inches in diameter through a thick piece of plate glass; on each side of this a thin piece of similar glass is laid, the trough having been filled with the solution under investigation. Troughs made in this manner never leak; they completely answer their purpose, and are easily washed and refilled.

958. Let the substance under trial be a concentrated solution of bichromate of potash. Having adjusted the lamps and filled the trough, set it before one of them, supporting it by a proper foot. On looking through the tube of the photometer, if the absorbent cell of the copper solution has been previously removed, the circle of light will be seen very brightly illuminated; one of its halves of a yellow tint, but not much less luminous than the other. The copper cell being now restored to its place, on looking again through the tube there is a striking contrast—one half of the circle is of a bright blue, but the other seems totally black; with solutions which cut off the blue rays less perfectly, this blackness is of course less intense; in these cases the lamps are to be moved into such positions that the two halves of the circle are equally illuminated, and its dividing diameter invisible. As the eye is not disturbed with any difference of colour, the observation can be made within small limits of error. The calculation of the relative intensities can then be made by the common photometrical law.

959. As this photometer is affected by rays of the same refrangibility as those which affect the tithonometer, it is clear that if it be the rays of light which are operative in the union of chlorine and hydrogen, the results given by the two instruments should correspond within certain small limits of error.

960. With respect to the tithonometer, I have improved this instrument considerably: by shading it with a glass case, which cuts off thermometric disturbance; by taking the observations through a small telescope, which avoids parallax; by having the scale movable, so as to slide along the tube; by making one charge of gas last for a great number of experiments; by completing the tithonization before commencing; by altering the position of the adjusting wire, so as to bring it nearly down to the end of the tube. The detail of these changes would, however, detain me now too long; they will be given at some suitable time hereafter.

961. I may, however, record as a striking fact, that so great is the sensitiveness of chlorine and hydrogen, that a mixture will actually explode by the rays of an electric spark. In Chapter XVI. I have already stated that silent combination would take place under these circumstances, but more recently I have had instruments repeatedly destroyed by explosions resulting in that way.

962. It being understood that the indications of the photometer and the tithonometer correspond, that they are affected by rays of the same refrangibility, we are enabled to proceed to the direct solution of the question, and the determination of M. Becquerel's hypothesis.

963. A transparent medium, which absorbs, to a greater or lesser extent, the more refrangible rays, being selected, it is required to determine whether, when a given ray has passed through it, the chemical effect diminishes as the intensity of the more refrangible rays of light diminishes. According to M. Becquerel, the effect should be in di-

rect proportion to the quantity of light; if, for example, the ray lost one half of its blue light, the chemical effect should diminish one half, &c.

964. We require, therefore, two observations: first, with the photometer, to ascertain how much of the light escapes the absorptive action of the medium under trial; second, with the tithonometer, to ascertain what quantity of the tithonic rays escapes absorption. If the whole effect is due to light, the two observations should give the same result.

965. Before giving the results which I have obtained in a tabular form, in order that I may be clearly understood I will give a particular example. I took some naphtha, the colour of which was slightly yellow, and placing it in the glass trough before described, proceeded to determine its relation for the more refrangible rays of light. This was done by adjusting the two lamps of the photometer till they coincided, then interposing the trough between one of the lamps and the end of the photometer. On looking through the tube, a great diminution of the intensity of the light on the corresponding semicircle of paper was observed; the other lamp was now removed until the paper disc was uniformly illuminated; the distance of the two lamps from the centre of the box was now measured—they were respectively twelve and fifteen inches; but the intensity of the light is proportional to the square of the distance.

(1.) For the light in the unobstructed beam . . . . . . 225 or 100.

(2.) For the light in the absorbed beam . . . . 144 or 64.

Supposing, now, that the value of the unobstructed beam be represented by 100, and we calculate the amount of light which passes through the naphtha, we find it is represented by 64; consequently, of every hundred rays of blue light which fell upon this naphtha, sixty-four escaped absorption.

The naphtha trough was now carried to the tithonometer; first it was determined how many seconds it took a given beam of light, coming from an Argand lamp, burning steadily, to move the index through one division.

(3.) For the tithonic ray of the unobstructed beam . . . . 31^s.

The trough was then interposed in the column of light, and the number of seconds required to make the index move through one division determined.

(4.) For the tithonic ray in the absorbed beam . . . . 65^s.

But as the lamp might have varied in intensity, or the tithonometer in sensitiveness, the first operation was repeated; it gave,

(5.) For the tithonic ray of the unobstructed beam . . . . 31^s.

This process of repetition was uniformly resorted to, and the mean of the two taken. It may be proper to remark, that there was rarely any perceptible difference.

966. It follows, that a ray which could effect the union of a given quantity of chlorine and hydrogen in 31 seconds, required, after passing through the naphtha, 65 seconds.

Calculating on these principles how many of the tithonic rays passed through the naphtha as before, we find,

(6.) For the tithonic rays in the unobstructed beam . . . . . . 100.

(7.) For the tithonic rays in the absorbed beam . . . . 48.

Now, comparing this result (6) and (7) with the result (1) and (2) for light, we find

that the absorptive action of naphtha, of a slightly-yellow tint, is very much greater for the tithonic rays than for the luminous rays.

967. Consequently, it follows that it is not the light comprehended between the extreme blue and extreme violet rays which brings about the union of chlorine and hydrogen, but another and invisible class of rays, which is absorbed by naphtha under a different law for that for its action on light.

968. The foregoing example shows the mode by which I have obtained the results of the following table:

TABLE SHOWING THE ABSORPTIVE ACTION OF CERTAIN MEDIA FOR THE LUMINOUS AND TITHONIC RAYS.

Name.	Number of rays which escaped absorption.	
	Tithonic rays.	Luminous rays.
Naphtha	48	64
Sulphocyanate of iron	66	58
Nitrate of iron	53	64
Red prussiate of potash	12	15
Turpentine	20	38
Copaiva balsam	24	42
Iodide of starch	40	33
Stevens's blue ink	30	33
Litmus water	16	23

From this table, therefore, we gather that the chemical effect produced by a given ray has no relation to the quantity of light which is in it; that a satisfactory explanation of the phenomena can only be given by assuming the existence and presence of another agent besides the light, and to which agent the chemical effect is due; that media are known which in their absorptive action bear relations which are totally different for these two agents; and, finally, that, as prismatic analysis has also previously shown, no explanation can be given of these results by imputing them to the agency of light, we are forced to admit the existence of another imponderable principle, the same as that which passes in these papers under the name of tithonic rays.

969. In addition to the results obtained from the foregoing quantitative experiments, there are other phenomena of a very novel and interesting kind, from which we may draw an argument of overwhelming force. The discussion of these I shall now take up.

970. Early during the last century, the remarkable appearance of phosphorescence excited in the Bolognean stone and calcined oyster-shells, attracted the attention of chemists. Dufay, in France, wrote several papers upon it; and the experiments of Wilson in England are, perhaps, as fine a specimen of philosophical investigation as those early times can furnish. These results, which few are now acquainted with, deserve to be republished.

971. To Becquerel we are indebted for one of the most remarkable discoveries in connexion with this subject. He found that the rays of an electric spark which had passed through glass no longer preserved the quality of exciting phosphorescence, but when they passed through quartz they retained that power unimpaired. This result is very strikingly shown by placing a piece of colourless glass and a piece of quartz on a surface covered with sulphuret of lime (oyster-shells calcined with sulphur), and discharging a Leyden vial a little distance off. The sulphuret will glow as brilliantly on the part covered by the quartz as on the uncovered spaces, but under the glass it wil. remain dark.

972. Nevertheless, this same sulphuret, carried into the sunshine, phosphoresces powerfully *under glass*, apparently showing that there is a difference between the phosphorogenic emanation of the sun, for thus M. Becquerel terms it, and that of an electric spark.

973. On this radiation, as it comes from the sun, M. Becquerel has treated in his paper, of which a translation is given in Taylor's Scientific Memoirs (vol. iii., part 12). He determines the place of the phosphorogenic rays in the spectrum after they have passed through a glass prism, and shows that the fixed lines which occur in the chemical spectrum, occur also among these phosphorogenic rays.

974. In passing, I may mention that, at the time I published my account of these fixed lines (*Phil. Mag.*, May, 1843), I had no idea that any other chemist had seen them. It, of course, soon appeared that M. Becquerel had some months previously given an account of them to the French Academy. My result was wholly independent, and without any knowledge of his. On comparing the two papers, it will be seen that there is a strong coincidence, not only in the manner of the experiment, but even in the very phrases of description. It is this which has drawn these passing remarks from me. Men who are pursuing the same object, and using the same resources, will employ even words that are alike, though they speak languages that are different, and live thousands of miles apart.

975. On examining the plate given in M. Becquerel's paper, I was struck with the close resemblance between the phosphorogenic spectrum and that tithonographic spectrum on iodide of silver of which Sir J. Herschel has given an elaborate account (*Phil. Mag.*, Feb., 1843). As far as the eye could judge, they seemed perfectly alike; the tithonographic spectrum in question was obtained by me in Virginia. This coincidence was so striking that it appeared almost certain that the phosphorogenic emanations of Becquerel were the same as my tithonic rays: there was the upper spectrum commencing at the line G, and going beyond the farthest confines of the violet, exerting a positive action; there was also the lower spectrum, commencing at the line F and going below the red ray, and exerting a negative action; a phenomenon absolutely the same as that traced on the Daguerreotype plate.

976. The phosphorogenic rays that come from the sun have the same place in the spectrum, or are dispersed by the prism exactly in the same way as the tithonic rays. To all appearance these may be expressions for the same agent.

977. We must remember, however, that the phosphorogenic rays of the sun differ from those of an electric spark. Glass to the former is transparent, to the latter it is not.

978. Before, therefore, I can carry this argument to the point on which I design it to bear, it is necessary to ascertain the index of refraction of the rays of an electric spark to which glass is impervious.

979. This I proceeded to determine in the following way: At the distance of six inches from the terminations of two blunt wires, between which the spark from a Leyden vial was caused to pass, I placed a lens of quartz, the focus of which for parallel rays was six inches, and then intercepted the resulting beam by a diaphragm with a circular aperture in it one third of an inch in diameter. I had caused an equiangular prism of quartz to be cut and polished from a large and perfectly faultless rock-crystal;

it was cut transverse to the axis. This prism I placed in such a way, that in dispersing the beam that came through the circular aperture I got rid of double refraction and obtained only one spectrum; this was received on a metal plate, which, having been washed over with gum water, and sulphuret of lime dusted on it, offered a uniform phosphorescent surface, which might be set in a vertical plane. *When the spark passed, I saw that the plate was phosphorescing on those portions where the more refrangible rays had fallen.*

980. But the transient light of a Leyden spark did not last long enough, nor was the phosphorescence it produced powerful enough to enable me to conduct the experiment in a way entirely satisfactory. I resorted, therefore, to the brilliant light which is obtained when a piece of metal, or, what is far better, the hard variety of carbon which is obtained from the interior of gas retorts, is lowered upon mercury entirely filling a very small open porcelain cup, and the continuous discharge of a voltaic battery passed. The battery used contained fifty pairs of Grove's cells, but a smaller number would probably have been amply sufficient. All the remainder of the arrangement was as just described.

981. As soon as the light was emitted, I marked on the sulphuret of lime the beginning of the red, the centre of the yellow, and the termination of the visible violet ray. Then, stopping the current, I examined on what parts the plate was phosphorescing. The commencement of the glow was between the indigo and the blue; towards the blue it extended far beyond the visible boundaries of the spectrum; I could not see any divisions or points of maxima on it. The surface of the plate shone all over, except in the region of the less refrangible rays, and there were the traces of the negative action which M. Becquerel has so well illustrated in the case of the solar emanations; rays which, however, were first observed in the last century.

982. It is necessary to remark, that the rays from the voltaic discharge resemble those from an electric spark in their inability to traverse glass. On this observation all the value of the foregoing experiment depends.

983. But it can nevertheless be easily proved, that although glass is impervious to the phosphorogenic emanation coming from the voltaic deflagration of any metallic bodies, the observation applies to transient discharges only. A voltaic light, which lasts but a moment, fails to cause phosphorescence through glass in the same way that an electric spark does; but if the discharge is continued, the surface presently begins to glow; and if maintained for several minutes, it shines as brightly as though a piece of quartz had been used.

984. The inability of an electric spark to cause phosphorescence is connected with its transient duration. The voltaic light enables us at pleasure to imitate the effects of an electric spark, or those of the sun.

985. The phosphorogenic rays, whether they originate in an electric spark or from the sun, occupying thus the same place in the spectrum, and even exhibiting the same peculiarities as the tithonic rays on the iodide of silver, we have next to determine whether this is an apparent or a positive identity.

986. Professor Henry, of Princeton, read a paper before the American Philosoph-

ical Society in May, 1843, in which he discussed all the leading mechanical properties of the phosphorogenic rays, and among other important experiments, made some with a view of determining this particular question. A Daguerreotype plate and some sulphuret of lime were simultaneously exposed to the sky; the plate was stained, but no effect was produced on the lime. A Daguerreotype plate and some sulphuret of lime were exposed to the light of an electric spark; the lime was observed to glow, but no impression was produced on the plate. When the plate was exposed to a succession of sparks for ten minutes, with a sheet of mica interposed, an impression was made. Lime exposed to the moon did not phosphoresce, but a sensitive plate under the circumstances is said to be stained. In view of these different facts, Professor Henry observes, "These experiments, although not sufficiently extensive, appear to indicate that the phosphorogenic emanation is distinct from the chemical, and that it exists in a much greater quantity in the electric spark than either the luminous or chemical radiation."

987. From Wilson's experiments, it appears that he was aware that when the phosphorescent surface is warmed, so as to hasten the disengagement of light, the moonbeams may be found to have left traces of action upon it; feeble, it is true, but nevertheless very apparent. We have seen, also, that the peculiarity of an electric spark is due to its transient duration. Before, therefore, a final decision can be obtained on this point, we are required to examine the effect of the tithonic rays and phosphorogenic emanation, under circumstances which are precisely similar as to intensity and time.

988. For the transient rays of an electric spark, quartz is transparent, and glass is nearly opaque. Having prepared a bromo-iodized silver plate so as to be exceedingly sensitive, I set in front of it, at the distance of about one third of an inch, a disc of quartz and one of crown glass, of equal thickness; and between a pair of copper wires, the interval of which was three eighths of an inch, I passed the spark of a Leyden vial fifteen times; the distance between this spark and the sensitive plate was about two inches. On mercurializing, the plate was deeply whitened all over, equally so through the glass, through the quartz, and on the uncovered spaces; but a spot of sealing-wax which I had put on the glass, left its shadow on the plate beautifully depicted, as were also the edges of the glass and the quartz. The two discs overlapped one another to a certain extent, but the corresponding portion of the silver plate was as deeply stained there as anywhere else.

989. Next I put a surface of sulphuret of lime in the place of the Daguerreotype plate, everything remaining as before. On passing fifteen sparks, the lime phosphoresced powerfully under the quartz, but not under the glass, so that the difference between its shadow and that of the spot of wax could not be distinctly seen.

990. For these reasons, therefore, I adopt the view expressed by Professor Henry, that the phosphorogenic emanation and the tithonic rays are distinct. Under the same circumstances, glass to the one is transparent, to the other it is opaque.

991. Now upon what sort of evidence is it that M. Melloni is universally admitted to have established the physical independence of light and heat? Was it not by show-

ing that rock-salt is perfectly transparent to calorific rays, that glass is much less so, that Rochelle salt, alum, and sulphate of copper are almost opaque? It is surely impossible to confound the phosphorogenic emanation of an electric spark with its rays of light; the latter pass perfectly through glass, the former do not. So far as the eye can distinguish, an electric spark, the rays of which have passed through glass, differs in no respect from one the rays of which are received directly into the eye. If we consider the constitution of such a ray, previous to and after its passage through glass, the eye can discover no difference; but, as respects the phosphorogenic emanation, there was something existing in that ray at the first of these epochs which had ceased to exist in it at the second; a something not having the quality of communicating any impression to the organ of vision; and that which we cannot see, surely no man will acknowledge to be light.

992. But the reader may inquire, What has all this discussion of the characters of the phosphorogenic emanation to do with the existence of the tithonic rays as a fourth imponderable? A few words will show. From these considerations and experiments, we have arrived at the conclusion that there exist in the beams of an electric spark *invisible rays, which are therefore totally distinct from light.* They occupy the same spectrum region as the tithonic rays which decompose iodide of silver; their leading character is, that glass, which is transparent to the rays of light, is opaque to them.

993. But the admission of this fact breaks down at once the doctrine of a trinity of imponderables, and compels us to enlarge our list of those living forces of chemistry. The great obstacle which is in the way of admitting the tithonic rays as a fourth imponderable, is in the circumstance that it would impress a very serious change on that science, and apparently afford an argument of weight against the mathematical theory of light. I believe that some great generalization will hereafter prove that all these imponderables are modifications of one primordial principle. I also believe that some capital experiment will hereafter show that the forty different metals we are acquainted with are merely modifications of one or two more simple forms; but these are things that we are unable to deal with now; and viewing the experiments which have been made in the last few years, not as mathematicians, but as chemists, all men must acknowledge that our prevailing doctrines of the nature and number of the imponderables are liable, before long, to undergo a very serious modification.

994. The admission that the phosphorogenic emanation and light are principles differing from the tithonic rays and from each other, relieves us of much difficulty in increasing our list of imponderables. If these principles differ thus intrinsically from one another, the question comes home to us, What are they? If electricity, and heat, and light are three recognised imponderables, are not the tithonic rays a fourth, *and the phosphorogenic emanation a fifth?*

995. In view of this, I would suggest the propriety of ceasing to call these last by the epithet of *emanations*, and of giving them the more appropriate name of PHOSPHOROGENIC RAYS.

996. And now, what appears to become of M. BECQUEREL's hypothesis, that all the different effects we have been considering are due to light, and are presented to us

under different aspects, because everything depends on the nature of the receiving surface; that it is the same principle which affects the eye as light, decomposes chloride of silver as a tithonic ray, and makes sulphuret of lime shine as a phosphorogenic ray; that the difference is not in the radiant principle, but in the surface on which it is received? To go no farther in a discussion which has already extended this chapter too much, if the agent is the same in all cases, and the difference perceived is due to the receiving surface, how is it that a ray of light which has passed through a piece of transparent glass can no longer excite phosphorescence in the sulphuret of lime? Can we escape the conclusion that the ray has had something removed from it, or has had some modification impressed on it, or, in short, that something invisible to the eye has been taken away?

997. To my mind these considerations are conclusive, *and I therefore regard the tithonic rays as constituting a fourth imponderable, and the phosphorogenic rays as a fifth.*

THE END.

Plate 1

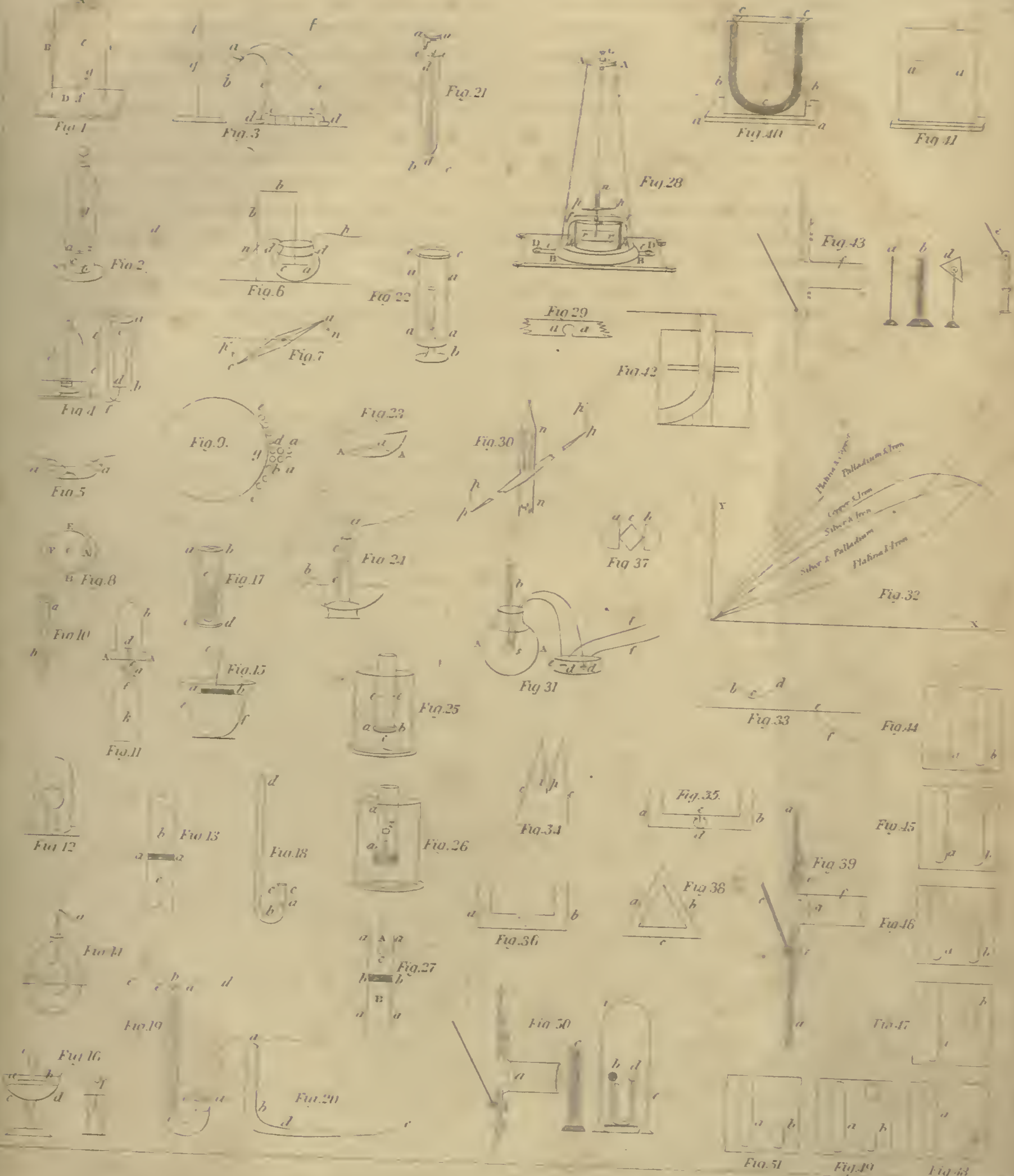
Fig.28
Fig.32
Palladium & Iron
Copper & Iron
Silver & Iron
Silver & Palladium
Platina & Iron

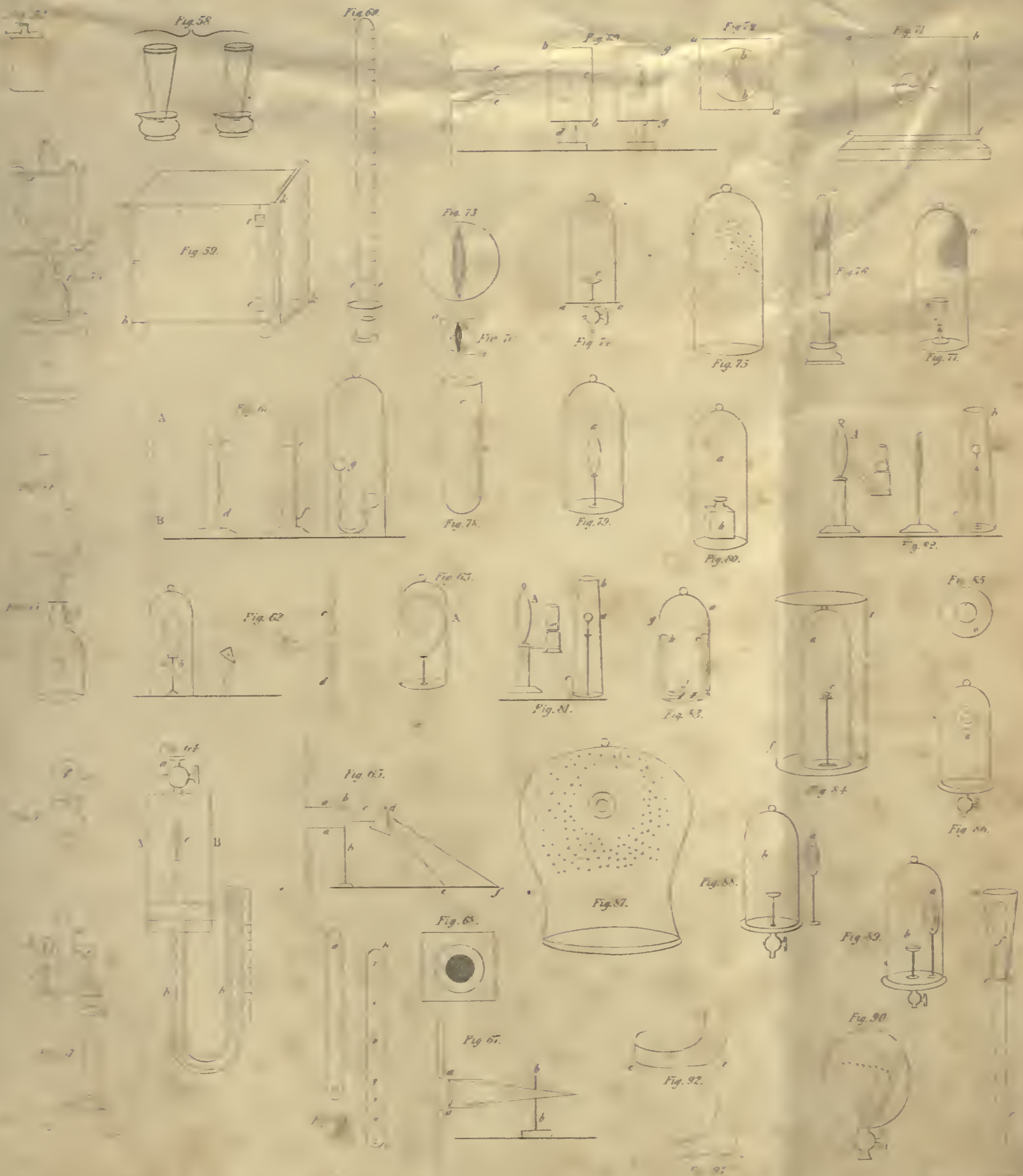
Fig. 58
Fig. 60
Fig. 71
Fig. 59
Fig. 73
Fig. 75
Fig. 77
Fig. 78
Fig. 79
Fig. 80
Fig. 62
Fig. 81
Fig. 85
Fig. 87
Fig. 88
Fig. 89
Fig. 90
Fig. 92

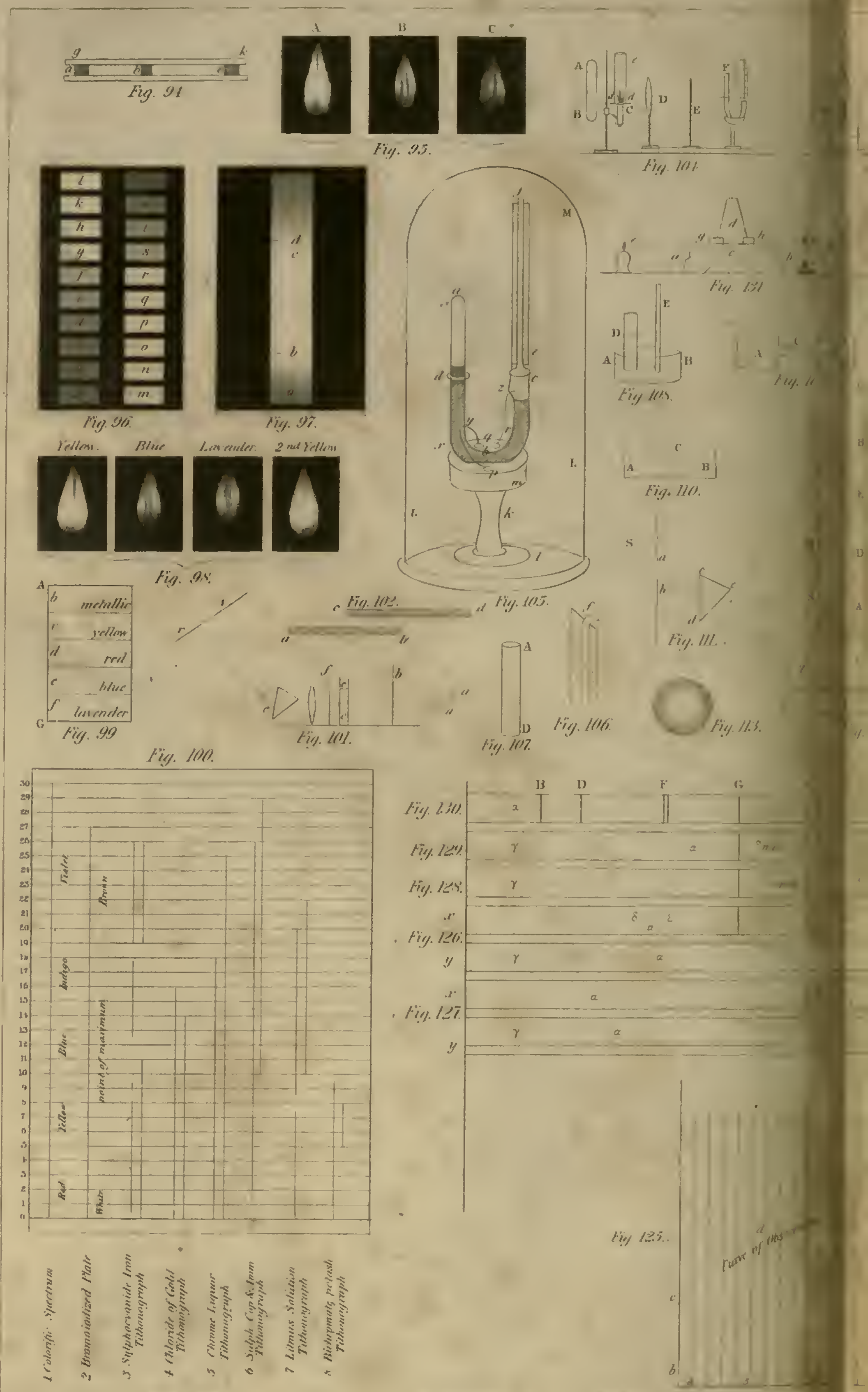
Fig. 94
Fig. 95.
Fig. 104
Fig. 96.
Fig. 97.
Fig. 105.
Fig. 108.
Fig. 110.
Yellow.
Blue
Lavender.
2nd Yellow
Fig. 98.
metallic
yellow
red
blue
lavender
Fig. 99
Fig. 102.
Fig. 101.
Fig. 107.
Fig. 106.
Fig. 111.
Fig. 113.
Fig. 100.
Violet
Indigo
Blue
Yellow
Red
Brown
point of maximum
White
1 Colorific Spectrum
2 Bromoiodized Plate
3 Sulphocyanide Iron Tithonograph
4 Chloride of Gold Tithonograph
5 Chrome Liquor Tithonograph
6 Sulph Cop & Amm Tithonograph
7 Litmus Solution Tithonograph
8 Bichromate potash Tithonograph
Fig. 130.
Fig. 129.
Fig. 128.
Fig. 126.
Fig. 127.
Fig. 125.

Plate III

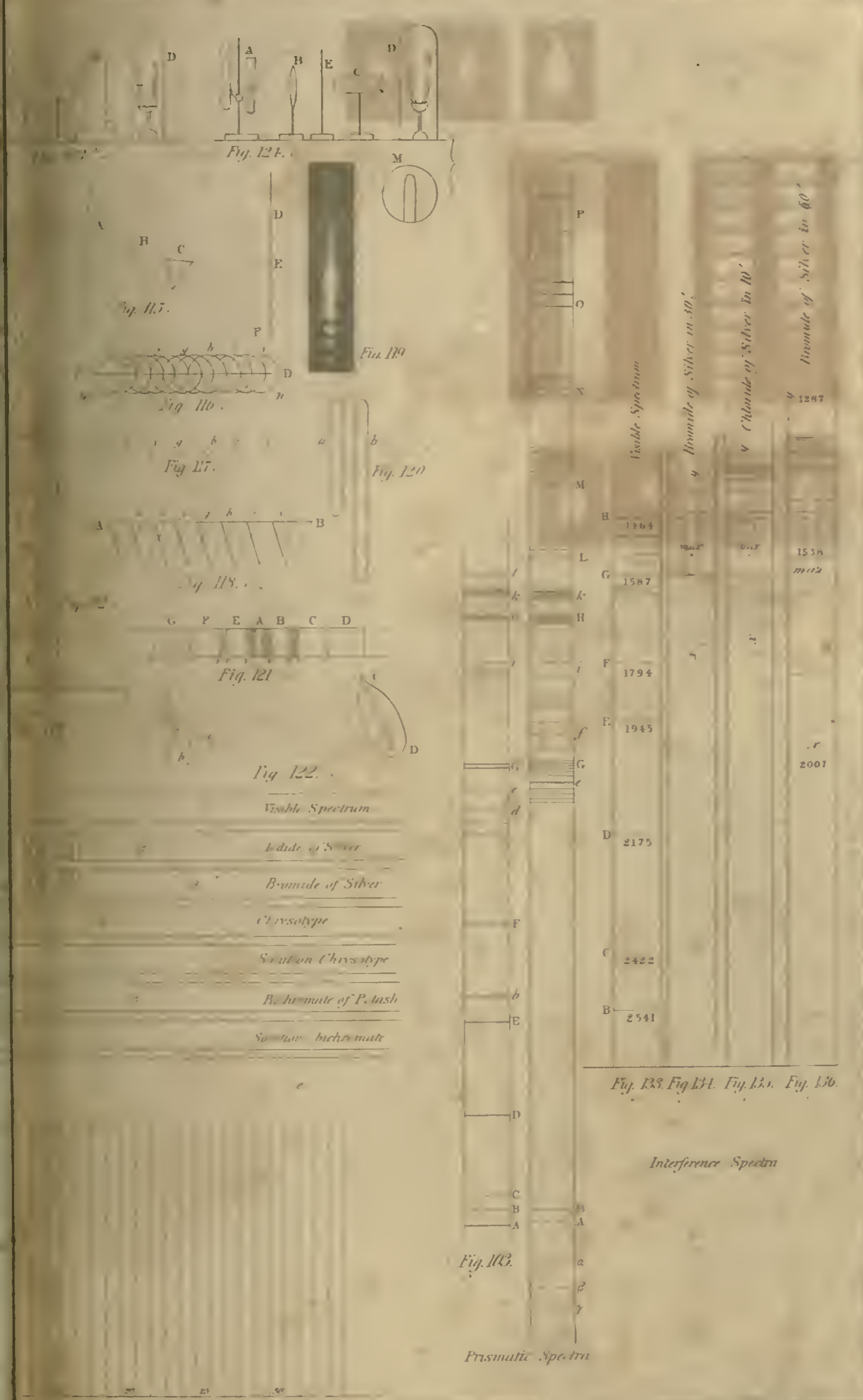

Fig. 124.
Fig. 115.
Fig. 119
Fig. 116.
Fig. 117.
Fig. 120
Fig. 118.
Fig. 121
Fig. 122.
Visible Spectrum
Iodide of Silver
Bromide of Silver
Chrysotype
Chrysotype
Bichromate of Potash
Fig. 103.
Prismatic Spectra
Visible Spectrum
Bromide of Silver in 30'
Chloride of Silver in 10'
Bromide of Silver in 60'
1287
1364
1538
1587
1794
1945
2001
2175
2422
2541
Fig. 133. Fig. 134. Fig. 135. Fig. 136.
Interference Spectra